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CONTENTS

Proceedings of the Thomas Young Memorial Meeting—Fourteenth Annual Meeting of the Optical Society of America – 139
Minutes of the Executive Council – – – – – – – – – 145
Professor Edward Leamington Nichols – IVES MEDALLIST for 1929 161
The Deflection of Light as Observed at Total Solar Eclipses – – – – – – – – – – – CHARLES LANE POOR 173
A Comparison of Laboratory and Solar Wave Lengths – – – – – – – – – – – – KEIVIN BURNS 212
The Observed Motion of the Perihelion of Mercury – – – – – – – – – – – HERBERT R. MORGAN 225
Index to Advertisers – – – – – – – – – – Cover III

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The Constitution provides that any one who has contributed materially to the advancement of optics shall be eligible to regular membership in the Society with the privilege of voting and holding office. Any individual or corporation interested in optics is eligible to associate membership. Annual dues including subscription to the JOURNAL are five dollars for individual associate membership, seven dollars and fifty cents for individual regular membership and fifty dollars for corporation membership. Further information may be obtained from the secretary.

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of the
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Vol. 20 APRIL, 1930 No. 4

Optical Society of America

Proceedings
of the
Thomas Young Memorial Meeting
(Fourteenth Annual Meeting)

CORNELL UNIVERSITY
ITHACA, N. Y.

October 24, 25, and 26, 1929

Portrait by Sir Thomas Lawrence

Thomas Young MD
&c. &c. &c.

This Meeting is Dedicated to the Memory of

Thomas Young

1773–1829

Eminent in

PHILOSOPHY, PSYCHOLOGY

PHYSICS, ASTRONOMY, MATHEMATICS

OCEANOGRAPHY, ENGINEERING, METALLURGY

MEDICINE, ART, ARCHEOLOGY, PHILOLOGY

in Appreciation of His

Outstanding Contributions

to

PHYSICAL, PHYSIOLOGICAL, AND
PSYCHOPHYSICAL OPTICS

Er war einer der scharfsinnigsten Männer, die je gelebt haben, hatte aber das Unglück, seinen Zeitgenossen an Scharfsinn zu weit überlegen zu sein. Sie staunten ihn an, aber konnten dem kühnen Fluge seiner Kombinationen nicht überall folgen, und so blieben eine Fülle seiner wichtigsten Gedanken in den grossen Folianten der Königlichen Gesellschaft von London vergraben und vergessen, bis eine spätere Generation in langen Fortschritte seine Entdeckungen wieder entdeckte und sich von der Richtigkeit und der Beweiskraft seiner Schlüsse überzeugte.

HELMHOLTZ

MINUTES OF THE MEETING

The fourteenth annual meeting of the Optical Society of America was held at Cornell University, October 24, 25, 26, 1929. This meeting was specially designated as the "Thomas Young Memorial Meeting."

The meeting convened Thursday morning, October 24, and, in the absence of the president of the University, the Society was welcomed to Cornell by Dean Robert Morris Ogden.

THOMAS YOUNG MEMORIAL SESSION

Thursday afternoon, October 24, was devoted to papers on Thomas Young and his work. The following papers were presented by invitation.

Thomas Young, the Man and Physician, by Prof. H. B. Williams.
Thomas Young's Theory of Color Vision, by Prof. Selig Hecht.
Thomas Young and the Wave Theory of Light, by Prof. Henry Crew.

On invitation of the president, Prof. G. P. Thomson of Aberdeen also spoke informally of Young at the close of this session.

AWARD OF THE FREDERIC IVES MEDAL TO PROFESSOR EDWARD LEAMINGTON NICHOLS

The society's jury on award of the first Frederic Ives Medal having awarded this medal to Professor Edward Leamington Nichols, the medal was presented to him by the president of the Optical Society at a dinner held in honor of Professor Nichols on Friday evening, October 25, which was attended by a great number of Professor Nichols' friends and colleagues. One hundred and ninety-one persons attended this dinner. (See separate article, p. 161, this issue.)

SESSION DEVOTED TO EXPERIMENTAL DATA BEARING ON THE THEORY OF RELATIVITY

The session Friday afternoon, October 25, was devoted to papers on the experimental data bearing on the theory of relativity. The following papers were presented by invitation.

The Deflection of Light as Observed at Total Solar Eclipses, by Prof. Charles Lane Poor.
A Comparison of Solar and Laboratory Wave Lengths, by Dr. Keivin Burns.
The Observed Motion of the Perihelion of Mercury, by Dr. H. R. Morgan.
Ether-Drift Experiments in 1929, by Prof. Dayton C. Miller.

By invitation of the program committee, Dr. W. F. Meggers also discussed the experimental data bearing on the relativity "red shift" of wave lengths; and read a discussion prepared by his colleague, Dr. C. C. Kiess, on the same subject.

PUBLIC LECTURE ON CONTEMPORARY THEORIES CONCERNING THE NATURE OF LIGHT

On Thursday evening, October 24, Dr. W. F. G. Swann gave, by invitation, a lecture on *Contemporary Theories Concerning the Nature of Light.*

COMMUNICATION ON PROPOSED STANDARDS FOR "WHITE LIGHT" FROM THE BRITISH COMMITTEE OF THE INTERNATIONAL COMMISSION ON ILLUMINATION

A large part of the Saturday morning session was devoted to a discussion of a communication from the subcommittee on colorimetry of the National Illumination Committee of Great Britain (affiliated with the I. C. I.). The detailed minutes of this session together with contributed discussion are to be published separately from these general minutes.

VOTE OF THANKS TO CORNELL UNIVERSITY AND THE LOCAL COMMITTEE ON ARRANGEMENTS

At the session Saturday morning, October 26, Dr. Herbert E. Ives moved a vote of thanks to Cornell University and the local committee for their hospitality and many courtesies which had contributed to a highly successful and enjoyable meeting. The motion was carried unanimously, and the secretary was instructed to communicate it to the President of the University and the local committee.

BUSINESS MEETING

A brief business session of the society was held at 2:40 p.m., Saturday, October 26, President Priest presiding.

Informal reports for the current year were presented by the secretary, the treasurer and the business manager of the journal. (Note: The society's fiscal year being identical with the civil calendar year, the officers' *formal* reports are made as of December 31 each year.)

The secretary presented the report of the tellers on the election of officers for the term beginning January 1, 1930 and ending December 31, 1932. The results were as follows:

President, L. A. Jones
Vice-President, E. C. Crittenden

Members of the Council:

A. H. Compton
I. C. Gardner
P. E. Klopsteg
E. D. Tillyer

These officers were declared elected.

There being no other business, the business session was adjourned, and the session for reading of papers transferred from the morning session was resumed.

ATTENDANCE

The total registration was:

Members	82
Guests	66
Total	148

The attendance at the several sessions was approximately as follows:

Thursday, October 24, 9:00 a.m.	120
Thursday, October 24, 2:30 p.m.	160
Thursday, October 24, 8:15 p.m.	290
Friday, October 25, 9:00 a.m.	150
Friday, October 25, 2:30 p.m.	400
Saturday, October 26, 9:00 a.m.	80
Saturday, October 26, 2:30 p.m.	40

One hundred and ninety-one persons attended the dinner in honor of Prof. Nichols Friday evening.

SESSIONS FOR THE READING AND DISCUSSION OF CONTRIBUTED PAPERS

Sessions for the reading and discussion of contributed papers were held as follows:

October 24, 9:00 a.m	Radiation and Luminescence
October 25, 9:00 a m.	Physical Optics and Instruments
October 26, 9:00 a.m.	Colorimetry
October 26, 3:00 p.m.	Special session for continuation of morning program.

The authors' abstracts of these papers are appended.

MEETINGS OF THE COUNCIL

Meetings of the executive council were held as follows:

Wednesday, October 23, 8:45 p.m.
Thursday, October 24, 9:30 p.m.
Friday, October 25, 12:40 p.m.
Saturday, October 26, 12:40 p.m.
Saturday, October 26, 3:30 p.m.

(*Signed*) L. B. TUCKERMAN, *Secretary*

MINUTES OF THE EXECUTIVE COUNCIL

Annual Meeting at Ithaca, N. Y., Oct. 23–26, 1929.

Beside the transaction of routine business the following items of general interest to the members of the Society were taken up.

1. The resignation of C. C. Bidwell as Secretary and the appointment of L. B. Tuckerman to fill the unexpired term was announced.

2. A new policy with respect to the publications. After a thorough discussion of the relations between the Optical Society of America and the Scientific Apparatus Makers of America the Council passed the motion:

The Optical Society of America in cooperation with the Scientific Apparatus Makers of America agrees to publish two journals,

(a) THE JOURNAL OF THE OPTICAL SOCIETY OF AMERICA.

(b) An instrument journal with the scientific standard of the REVIEW OF SCIENTIFIC INSTRUMENTS, but accompanied by more popular features, under such conditions as may be agreed upon by the Committee on the Contract with the Scientific Apparatus Makers of America.

To this was added the stipulation that in the case of a paper on Optical Instruments the author should have the privilege of deciding in which journal it should be published.

3. The publication of a new directory of members. The council decided to publish an alphabetical list of the members, the officers, committees, past officers, and any amendments to the By-Laws as early in 1930 as it could be prepared.

4. Printing of Programs. The Council decided that in the future the programs of Meetings should be issued by the Secretary and not as a supplement to the Journal. Although involving somewhat greater expense it would make possible a later closing date.

5. Transfers from Associate to Regular membership. The name of each associate member with the recommendations from the Committee on Transfer were taken up in order, and the following members were transferred to Regular Membership:

FRANCIS H. ADLER (677 R)
HAROLD L. ALDEN (711 R)
ARTHUR J. AMES (696 R)
WILLIAM D. APPEL (599 R)
ROBERT F. ARNOTT (698 R)
JOHN M. ARTHUR (683 R)
FRED W. ASHTON (593 R)
A. LATHAM BAKER (697 R)

CARL L. BAUSCH (681 R)
LEO BEHR (426 R)
WILLIAM BERRY (675 R)
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J. EDWARD PATTERSON (117 R)
ROBERT A. PATTERSON (670 R)
MARCELLO PIRANI (662 R)
SAMUEL E. POND (412 R)
A. D. POWER (744 R)
R. H. RANGER (439 R)
HORACE C. RICHARDS (618 R)
SAMUEL H. ROBERTS (584 R)
THOMAS W. ROLPH (477 R)
WILLIAM EWING SHAHAN (708 R)
JAMES F. SKOGLAND (606 R)
C. M. SLACK (580 R)
WILLIAM W. SLEATOR (752 R)
LOUISE L. SLOAN (387 R)
CARROLL M. SPARROW (641 R)
JOEL STEBBINS (730 R)
LEROY W. TILTON (647 R)
ALFRED TRAEGER (430 R)
CLIFFORD L. TRELEAVEN (543 R)
FREDERIC GORDON TUTTON (581 R)
C. T. ULREY (521 R)
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HENRY T. WENSEL (657 R)
ADOLFO T. WILLIAMS (621 R)
LUCY WILSON (672 R)
GEORGE WINCHESTER (653 R)
FREDERIC A. WOLL (664 R)
W. D. WRIGHT (714 R)
ZIRO YAMAUTI (741 R)

[*Signed*] L. B. TUCKERMAN *Secretary*

PROGRAM OF SESSIONS FOR CONTRIBUTED PAPERS

(TITLES AND ABSTRACTS OF PAPERS)

The abstracts were preprinted in the program subject to correction by the authors. Corrections that have been communicated to the secretary have been made in the abstracts reprinted below.

Thursday, October 24
9:00 A.M.

CONTRIBUTED PAPERS ON RADIATION AND LUMINESCENCE

E. L. Nichols — **Cornell University**

REVERSIBLE OXIDATION IN LUMINESCENCE

In this paper numerous instances are cited in the fields of cathodo-photo-and chemi-luminescence which favor the view that reversible oxidation is the general and perhaps the universal cause of luminescence.

The results of new experiments on:

(1) *The effects of ozone in thermo-luminescence,*

(2) *Activators in cando-luminescence,*

are shown to be strongly confirmatory of this hypothesis.

This paper will appear in full in J.O.S.A. & R.S.I.

E. L. Nichols and L. J. Boardman — **Cornell University**

ACTIVATORS IN CANDO-LUMINESCENCE

The remarkable effects of traces of certain substances in rendering solids fluorescent are shown in the experiments described in this paper to be completely paralleled in the luminescence of incandescent oxides.

The following points are established in this investigation:

As in fluorescence so in cando-luminescence:

(1) The same activators are effective: notably oxides of Cu, Mn, Cr, etc. and the rare earths.

(2) The activator must be in solid solution.

(3) Enhancement increases as the proportion of activator decreases to a definite point (the optimum) then diminishes.

(4) Activation is highly selective; no known solid responds to all activators and no known activator is effective with all solids capable of luminescence.

This paper will appear in full in J.O.S.A. & R.S.I.

D. T. Wilber — **Cornell University**

(*Introduced by E. L. Nichols*)

LUMINESCENCE FROM CATHODE RAYS

An investigation of the behavior of the "pure" oxides of thirty three elements under cathode rays in a good vacuum shows (1) The alkalies are rapidly decomposed and give the arc spectrum, (2) The earths give luminescence without chemical decomposition, (3) The silicon group gives luminescence with slow decomposition, (4) The elements of higher valence give no luminescence but many are decomposed. Some of the latter seem to recombine rapidly under these conditions.

In many cases a bright luminescence at or near the surface appears to be due to the luminescence of the gas evolved, for example the bright blue glow appearing on powdered metallic tungsten as it is degassed by the cathode rays.

The simplest theory of the mechanism of light production in true cathodoluminescence is that the discharge decomposes the molecule which recombines producing light in a similar manner to the magnesium flash light which is clearly oxidation. Continuous oxidation and reduction have already been postulated to explain the luminescence due to the hydrogen flame. Reoxidation may be very slight as in sodium, nearly complete as in zirconium or may take place without the evolution of light as with the higher valence oxides.

An additional activator for alumina is found in manganese which results in a bright green luminescence. (The author acknowledges assistance from the Kecksher Research Council of Cornell University.)

Frank E. Swindells **The Patterson Screen Co.**

FACTORS INFLUENCING THE SPEED OF X-RAY INTENSIFYING SCREENS

The intensification, or speed factor of intensifying screens is found to increase with shortened development time; to increase with increase of tube potential; to be independent of the tube current from 2 to 35 milliamperes; to be independent of the target-film distance from 0.5 to 2.5 meters; to increase as more filtering medium is interposed between the tube and screens; and to vary considerably with the photographic emulsion used.

Bibliography:
R. B. Wilsey, Radiology, *2*, p. 311; 1924.
F. E. Swindells, Am. Roentgenology and Rad. Ther., *17*, p. 115; 1927.
F. E. Swindells, Radiology, *11*, p. 424; 1928.
The present paper will appear in full in J.O.S.A. & R.S.I.

H. T. Wensel, Wm. F. Roeser, L. E. Barbrow, F. R. Caldwell **Bureau of Standards**

THE WAIDNER-BURGESS STANDARD OF LIGHT

Following out the suggestion made in 1908 by Waidner and Burgess the luminous intensity of the primary photometric standards of the Bureau of Standards has been compared with the brightness of a black body at the freezing point of platinum.

The black body standard was realized experimentally by immersing a tube of fused thoria in a bath of purest platinum (purity higher than .999996) contained in a crucible of fused thoria. The platinum was melted in a high frequency induction furnace and observations of brightness were taken during the period of freezing.

The brightness of the Waidner-Burgess Standard was found to be 58.8 International Candles per sq. cm.

This paper will appear in full in Bureau of Standards Journal of Research.

W. E. Forsythe and F. L. Christison **Lamp Development Laboratory, Incandescent Lamp Dept., General Electric Co.**

ULTRA-VIOLET RADIATION FROM THE SUN AND FROM HEATED TUNGSTEN

There have been several investigations of the radiation characteristics of tungsten made at the laboratories at Nela Park. Making use of the relations thus found the spectral distribution of energy radiated by tungsten at different temperatures and different wave lengths has been calculated. By step by step integration the intensity of this radiation in different wave length regions was determined. The values used for the energy transmission of the earth's atmosphere and the distribution of energy in the sun's spectrum outside the earth's atmosphere were the published values of Abbot and his co-workers,[1] supplemented for the very short

wave lengths by the data of Fabry and Buisson.[2] Using the step by step method of integration the intensity of the radiation from the sun has been calculated for different wave-length regions. From the elevation of the sun and the different air masses involved the intensity of the radiation from the sun at different times of day and different seasons of the year has been calculated for different wave-length regions.

Source	Illumination (foot-candles)	Total energy (milliwatts per cm²)	Ultraviolet energy falling on a cm² of surface (milliwatts)	
			Below 0.325μ	Between 0.31 and 0.29μ
Sun[a] 1 atm.	8540	107	0.26	0.024
1.07 atm.[b]	7900	97.6	0.21	0.019
1.5 atm.[c]	4940	64.0	0.07	0.0041
2.27 atm.[d]	2415	33.9	0.010	0.00017
500-watt tungsten[e] lamp 1000 hr. life	71	4.0	0.0013	0.00044
500-watt tungsten lamp, 100 hr. life	87	4.0	0.0017	0.00060
900-watt movie	292	9.2	0.0076	0.0028
Tungsten arc[f]	544	11.4	0.030	0.0108

[a] For a horizontal surface.

[b] June 21 for latitude of Cleveland (0.).

[c] About February 15 for latitude of Cleveland.

[d] December 21 for latitude of Cleveland.

[e] Values for the tungsten filament lamps and arc for a cm² at a distance of one meter from the lamp.

[f] Calculated for one cm² of molten tungsten.

Bibliography:

[1] Smithsonian Miscellaneous Collection, *74*, p. 15; 1923. Annals Astrophysical Observatory, *2*, p. 112; 1908.

[2] Comptes Rendues, *175*, p. 156; 1922. Astrophys. Jour., *54*, p. 297; 1921.

The present paper will appear in full in General Electric Review.

A. H. Pfund and H. B. DeVore — **DuPont Research Laboratory**

SPECTRAL SENSITIVITY OF PHOTOCHEMICAL DECOMPOSITION OF ORGANIC FILMS BY ULTRAVIOLET LIGHT

Quartz plates were coated with films of varnishes, etc., and were exposed in a quartz spectograph to the radiations of a quartz mercury arc—the films being used as photographic plates of low sensitivity. Of the variety of criteria for decomposition viz.: acidity, blushing, yellowing, etc., the latter was chosen because of its wider applicability. Graded exposures ranging from 24 hours to a few minutes were made for each film. The yellowing developed was so slight as to be imperceptible in daylight. However, by resorting to "ultraviolet printing" with radiations around 3100 A.U., remarkable photographs of the mercury spectrum were obtained. Having measured the energies of the various mercury lines with a vacuum thermopile and having determined the absorption coefficients for the various films throughout the spectrum, it was comparatively simple to determine the (yellowing) threshold intensities for equal energy of absorbed radiation. While the spectral sensitivity curves for the various films differ, they all reveal little sensitivity for radiations longer than 3300 A.U. The maximum lies at 3000 A.U. for an ordinary varnish and at 3100 A.U. for a four-hour varnish.

If curves be plotted for "equal yellowing" rather than for threshold, the sensitivity maxima shift toward longer wave-lengths. This is readily accounted for. Experiments bearing on the nature of the yellow film are presented.

Brian O'Brien — **J. N. Adam Memorial Hospital**

SEASONAL VARIATION OF CERTAIN PHOTO-CHEMICAL EFFECTS OF SUNLIGHT

Using spectral energy curves for the ultraviolet end of the solar spectrum (O'Brien, Phys. Rev., *33*, p. 1072; 1929), and the spectral sensitivity of erythema reaction in human skin (Hausser & Vahle, Strahlthpie, *13*, p. 41; 1921, Hausser, Strahlthpie, *28I*, p. 25; 1928, of bactericidal reaction, protein coagulation, and methaemoglobin reaction (Sonne, Strahlthpie, *28I*, p. 45; 1928), the integrated reactions to sunlight have been computed. The spectral sensitivity curve for the activation of ergosterol has been estimated from its absorption spectrum. All the above reactions approach zero for $\lambda > 3150$A. Thus the product of reaction per unit intensity and intensity approaches zero for $\lambda > 3150$A, and also for wave lengths less than the limit of the solar spectrum. If this product is plotted against wave length, the area of the curve represents the intensity of the reaction.

For exposure at noon, very clear days, lat. 42°27′ N. elev. 1480 ft., if each reaction be taken as 100 units on June 21st, the reactions on Mar. 21st and Dec. 24th respectively are: erythema 32.7, 2.51, bactericidal 25.1, 1.64, protein coagulation 36.7, 3.19, methaemoglobin 38.0, 3.54, ergosterol activation, 36.0, 3.15. For sunlight filtered through a good sample of ultraviolet transmitting glass (thickness 2.1 mm, transmission 42% at 3022A), the reactions in the same units on June 21st, Mar. 21st, and Dec. 21st respectively are: erythema 52.4, 18.6, 1.51, bactericidal 46.2, 12.8, 0.94. For sunlight filtered through a very good sample of ordinary window glass (thickness 2.0 mm, trans. 0.4% at 3022A, 51% at 3300A), the reactions on June 21st are: erythema 2.40, bactericidal 1.23.

The large seasonal variations in the magnitude of the reactions are evident, particularly the reduction near the winter solstice. It is of interest to note that direct sunlight on Dec. 24th produces little more reaction than sunlight on June 21st filtered through the above sample of ordinary window glass.

The computed reactions under sunlight have been confirmed experimentally in the case of erythema reaction.

Dr. George H. Maughan — **Cornell Medical College**

THE AMOUNT OF ULTRAVIOLET RADIATION NECESSARY TO CURE RICKETS AND THE AREA OF THE BODY THROUGH WHICH THE RAYS ENTER IT

In this paper the author gives the results of experiments conducted to determine the amount of radiation needed to cure rickets in chickens. The area of the body through which the rays enter it was also investigated.

Rickets in chickens can be cured with such uniform results that it can be compared in exactness to a chemical reaction. Because of this fact these animals are valuable in studying ultraviolet problems.

The time necessary to cure severe rickets is approximately twenty-eight days. It requires about one minute daily exposure at 22½ inches lamp-floor distance, or two minutes at 30 inches, to cause 95% cure, when the amperage is 4½ and the voltage is 75 across the arc of a new D. C. Cooper Hewitt quartz mercury burner. Beyond a certain maximum, which is perhaps double these figures, longer treatments show no added benefit.

It seems that there is little difference in rate of recovery if a total of 37½ minutes of exposure is given in four doses of equal length or in twenty-eight daily exposures of 1⅓ minutes each.

Chickens irradiated from the side require less exposure than those irradiated from above.

This difference is likely due to less shading of the feet when the lamp is at the side of the chickens.

Whenever feathers cover the animal as they do the body of chickens six to ten weeks old, they prevent the rays from reaching the skin. The parts which do absorb the vital rays are principally the feet and combs.

Male birds, because of larger combs, usually recover from the disease with less exposure than the females..

Friday, October 25
9:00 A.M.

CONTRIBUTED PAPERS ON PHYSICAL OPTICS AND INSTRUMENTS

Charles R. Larkin — **Lehigh University**

ELECTRIC DOUBLE REFRACTION IN LIQUIDS

Pauthenier's[1] work on electric double refraction in liquids (Kerr effect) has been repeated with some slight modifications. By the operation of suitably designed and adjusted electrical circuits the plates of the Kerr condenser are charged and the light passed through and extinguished in about 10^{-6} sec. In an interval this short the mechanical electro-strictive effect has not had time to set itself up and is thus eliminated from the result. The change in index of refraction due to heating of the dielectric is held at a minimum by repeating the process only often enough for the eye to integrate the result into a continuous image. The change in optical path due to the field was measured by a Jamin type interferometer. Measurements have been taken for several wave lengths of light between 5000A and 6000A, the strong groups of air lines from a spark being isolated for the purpose. Several liquid dielectrics were used with various impressed potentials up to 10^5 volts/cm. Especial precautions and procedure were found necessary for purifying the dielectrics used. In each case so far studied it has been found that the light whose electric vector is parallel to the field is retarded while the light whose electric vector is perpendicular to the field is advanced, their ratio being -2 within the limit of error of the observations which agrees with theoretical considerations.

Bibliography:

E. F. Kingsbury — **Bell Telephone Laboratories, Inc.**

THE KERR ELECTROSTATIC EFFECT

The Kerr electrostatic effect has been used recently in a light valve for receiving telephotographs. When an electric field is applied to two parallel plates immersed in a liquid such as nitrobenzol, there are developed between them planes of different refractive indices in the direction of the field and at right angles to it, the difference being proportional to the square of the field. This in general produces elliptic polarization when plane polarized light travels across the field, the axial diameters and positions relative to the field becoming functions of its strength. This is summarized for the various possible adjustments of the system.

Curves are shown of the variation of the light transmission through the analyzer as the voltage is applied for the various polarizer angles and the result of replacing a monochromatic light by white light is discussed. Experimental data obtained with the Kerr cell are compared with the calculated curves. As the system is ordinarily set the transmission is proportional to the square of the sine of the product of a constant and the electric field squared. A better fit is obtained to the experimental data if the exponent to the sine is made greater than 2.0.

For use in telephotography the light transmission characteristic has a restricted exposure range and also considerable curvature at the lower values necessitating an adjustment of the minimum exposure on a flat portion of the characteristic and the inclusion of a non-linear

[1] M. Pauthenier: Ann. de Physique, *14*, p. 239; 1920.

section within the operating range. It is important to use nitrobenzol of low electrical conductivity. Nitrobenzol strongly absorbs ultraviolet and blue light up to about 4400A which decreases its photographic efficiency.

This paper will appear in full in J.O.S.A. & R.S.I.

H. E. Ives and A. L. Johnsrud — **Bell Telephone Laboratories, Inc.**

TELEVISION IN COLOR BY A BEAM SCANNING METHOD

The object is illuminated by a rapidly moving spot of light, as in monochrome television by beam scanning. Three sets of photoelectric cells are used to pick up the reflected light and produce electrical signals. Each set of photoelectric cells is provided with color filters, so that the response corresponds to one of the three primary color elements, red, green, or blue. Three communication channels are used in place of one. At the receiving end two argon lamps with color filters are used for the green and blue lights, a neon lamp with filter for the red light. A system of semi-transparent mirrors superpose the light from three colored lamps on the eye through a scanning disc pierced with a spiral of holes, on the surface of which the colored image is observed. The conditions necessary for correct color rendering are discussed.

This paper will appear in full in J.O.S.A. & R.S.I.

W. W. Sleator — **University of Michigan**

THE PROPAGATION OF ENERGY BY WAVES

If rope waves or plane sound waves travel along the x axis, and u is the displacement of a bit of the medium from its equilibrium position, then u is a function of x and t (the time) and $du=(\partial u/\partial x)dx+(\partial u/\partial t)dt$. If $du=0$, $dx/dt=S$, the velocity of the waves, and $S=(-\partial u/\partial t)(\partial u/\partial x)=$velocity of medium-condensation. In sound waves or in rope waves any small volume of the medium has *always* equal amounts of kinetic and potential energy ($1/2ds^2=1/2$ stress$\times$strain) and this equality gives S above as $\sqrt{F/m}$ for rope waves and $\sqrt{n/d}$ for sound waves.

In plane waves of light the magnetic and electric fields, representing equal amounts of energy, are together in phase. The question arises, may one write these two fields as time and space derivatives respectively so as to obtain the expression $S=c/\sqrt{e}$ for the velocity of light? Having regard for the vector nature of E and H it is found possible to define a vector quantity u such that Curl $u=D$, electric induction, $\partial u/\partial t=H$, magnetic field, u may be called the "light vector" and its maximum value u is properly the amplitude of the wave. Using this quantity it appears that $S=H/D$, and the equality of energies gives at once $S=c/\sqrt{e}$. The use of U as amplitude gives energy transmitted as proportional to square of frequency. The field of U about an isolated charge has axial symmetry.

G. W. Moffit — **Yerkes Observatory**

ON THE MOUNTING OF CORRECTOR LENSES IN VISUAL TELESCOPES USED WITH STELLAR SPECTROGRAPHS

The zero power lenses used to adapt the color correction of telescopes designed for visual observation to the requirements of the stellar spectrograph, have certain unavoidable optical limitations. It has often been shown that in order to minimize the effects of these limitations it is better to use correctors of large diameter placed at correspondingly greater distances from the focal plane.

Usually the mechanical difficulties involved in mounting a corrector lens at a point far up in the tube of a telescope in such a manner that it may be quickly and easily removed or put in place, have led to the use of small correctors mounted rather close to the focal plane.

The mounting of a new corrector at 2½ meters distance from the focal plane in the 40-inch Yerkes telescope will be described as illustrative of a method of mounting that is convenient in operation, and meets the conditions for improved results.

Arthur C. Hardy **Mass. Institute of Technology**

THE DISTRIBUTION OF LIGHT IN OPTICAL SYSTEMS

The distribution of light in optical systems is considered for the case of a source obeying Lambert's law and an optical system obeying the sine condition. As the sine condition can be derived from the conservation of energy principle, such a system represents an ideal case, on the basis of which the efficiency of any actual system can be rated. The method is applied to the analysis of certain well known optical systems composed of spherical or cylindrical lenses or both.

This paper will appear in full in The Journal of the Franklin Institute.

Joseph Razek and Peter J. Mulder **University of Pennsylvania**

(*Introduced by C. E. Bazzoni*)

A BRIDGE GRID RESISTOR AMPLIFIER

A preliminary report on this work was given at the December 1928 meeting of the American Physical Society. The general equations for the two tube vacuum tube bridge in which grid resistors are employed have been developed. These equations indicate the factors which determine the sensitivity of the bridge.

The conditions to be satisfied in order that the galvanometer current remain steady at high sensitivity are derived from the same equations. These conditions are that the effective characteristics of the tubes be identical in pairs. In practice, this is almost impossible of realization.

A circuit arrangement is given for which these conditions are reduced in number and of such character as to be easily attained. It is shown that stability can be realized even though the tubes differ considerably in their characteristics.

Since it is generally impossible to measure the characteristics with sufficient precision, an experimental procedure for the rapid determination of the operating points for maximum stability is given. Various precautions to be observed in the use of the circuit are described.

The circuit has been used by the writers to amplify photoelectric currents in connection with a spectrophotometer. Many other uses are suggested.

To eliminate the necessity of any batteries, the circuit has been arranged for use on 110 volt A.C. The grids and plates are then supplied from an electron tube rectifier, and the filaments are supplied directly from a transformer. In this form the circuit is used in the recording and indicating Color Analyzer.

Bibliography:

Wynn-Williams, Proc. Cam. Phil. Soc., *23*, p. 811; 1927.
Eglin, J.O.S.A. & R.S.I., *18*, p. 393; 1929.
Mulder and Razek, J.O.S.A. & R.S.I., *18*, p. 466; 1929.
This paper will appear in full in J.O.S.A. & R.S.I.

F. G. Cottrell, C. H. Kunsman and R. A. Nelson **Bureau of Chemistry and Soils**
U. S. Department of Agriculture

A NEW METHOD OF PRODUCING AND CONTROLLING THE EMISSION OF POSITIVE IONS

A new method for the production and for controlling the emission of positive ions of potassium will be described. The essential part of the apparatus or anode is constructed by fusing a thin-walled potassium glass tube about a platinum strip making sure that a gas-tight seal exists between the glass and the platinum at the two ends. This is now placed in molten potassium nitrate and a considerable amount of potassium electrolyzed through the glass onto the platinum strip. A thin coating of platinum is sputtered onto the glass by means of a discharge, with the exception of short lengths near the two ends, which are kept free in order to insulate the platinum surface from the central platinum strip. This assembly, when

mounted as an anode in a vacuum tube and heated to 625°C by passing a current through the central platinum strip, gives off currents of positive potassium ions to 10^{-6} amperes cm.^{2} The emission obeys Richardson's equation. By varying the potential of the platinized emitting surface with respect to the central platinum strip the positive ion current may be varied over a wide range.

The advantages of this source over other sources is quite obvious, in that (1) the emission can be made very pure, i.e., a single alkali as potassium and not a mixture of ions, (2) a large amount of alkali can be electrolyzed onto the platinum strip which assures no decrease in current with time at a constant temperature, and (3) at a given temperature the positive ion currents can be changed by simply varying the potential of the emitting surface with respect to the potential of the hot strip. This relatively low temperature catalytically inactive surface is essential in experiments on gaseous activation by ions where one wishes to reduce the effect of the hot surface to a minimum.

K. C. D. Hickmman **Eastman Kodak Company**

MINIATURE CONDENSATION PUMPS

The efficiency of the Langmuir condensation pump has never been questioned but its utility has been limited by the necessity for water cooling and a liquid air trap.

Both limitations may be removed if a liquid less volatile than mercury is used as the activating fluid. During the last ten years many patents have been granted for pumps using lead, tin, and volatile salts but no mercury substitute has proved satisfactory until the recent introduction of high boiling hydrocarbons by C. R. Burch.

In the present paper an account is given of a number of pure organic liquids which have been tried in place of Burch's hydrocarbons in the diffusion pump. Diffusion pumps are generally too large for the work they have to perform. The capacity of the pump is limited by the connecting tubing and where this must be of small capacity the pump may be made so tiny that the heat losses are relatively great and water cooling may be avoided.

A miniature pump is described to be operated with n-di-butyl phthalate or butyl benzyl phthalate which will yield pressures as low as 10^{-6} mm without a trap and pressures beyond measurement with the ionization gauge when using a trap immersion in ice and salt.

This paper will appear in full in J.O.S.A. & R.S.I.

At the end of this paper there was a demonstration of the miniature condensation pump and of the Vacuum Recording Gauge. J.O.S.A. April 1929.

Saturday, October 26
9:00 A.M.

CONTRIBUTED PAPERS ON COLORIMETRY

F. F. Rupert **American Crayon Co. and Mellon Institute**

A STUDY OF PIGMENT PRIMARIES AND MIXTURES

The use of the spectrophotometer provides the best method for the study of the mixing properties of pigments. In transmission media such as solutions and films, the optical laws by which the effect of mixing may be predicted are simple, but in the case of reflecting surfaces there are many complications. A study of curves obtained from water color washes and dry pigments reveals no simple mathematical relations, but the following principles are established:

(1) When the reflectances of the components, at any given wave length, are equal or nearly equal, the reflectance of the mixture agrees closely with that predicted by formulas applying to transmission; (2) when the reflectances of the components are widely different, the reflectance of the mixture is nearer that of the lower component than predicted. The

mixing properties of any pair of chromatic pigments can thus be predicted with fair accuracy. A brief study of oil paints was also made, with similar results in some cases, but the optical properties of the vehicle often introduce additional complications.

Primary art colors may well be defined as the colors of the smallest number of tinctorial media chosen so that any due with satisfactorily high brilliancy and saturation may be produced by a mixture of not more than two of the media. On the bases of this definition, color analyses of all types of pigments, and the principles given above, it is shown that the broadest sense of the word "satisfactory" calls for three primaries, and that if three are chosen, they must be red, yellow and blue, with narrow limitations imposed upon the form of the spectrophotometric curves of the pigments used.

Clayton H. Sharp **Electrical Testing Laboratories**

MEASUREMENT OF FILAMENT TEMPERATURE OF INCANDESCENT LAMPS

The apparatus has been designed primarily for matching the color of an incandescent body with that of an incandescent lamp calibrated as a standard of color temperature. Use is made of a rotating disc one-half of which is red and the other blue, together with a photoelectric cell connected to an amplifier train in a circuit of such character that the red filtered impulses are opposed to the blue filtered impulses. A condition of balance is reached for only one degree of incandescence but with any other degree of incandescence a balance may be reached by interposing a filter of either bluish or yellowish tint and of the right density. This filter may conveniently take the form of a color wedge.

After a balance is had with the lamp to be measured, the calibrated lamp is substituted for it, and its voltage varied until balance is restored. The corresponding color temperature is read from the calibration curve or the voltmeter may be fitted with a scale reading color temperature directly. The applicability of the method to recording and to controlling devices is pointed out and also the possibilities of its usefulness in matching paints which are of the same basic composition.

Peter J. Mulder and Joseph Razek **University of Pennsylvania**

(*Introduced by C. B. Bazzoni*)

A PORTABLE RECORDING AND INDICATING COLOR ANALYZER

A portable recording and indicating color analyzer has been developed and built at the University of Pennsylvania. This first model consists of an ilumination unit, a spectrometer, a photoelectric cell connected to a bridge grid resistor amplifier, and a recording and indicating galvanometer. These parts are bult into a box about the size of a suitcase.

The illumination unit consists of a number of small bulbs arranged on a hemispherical surface, while the sample to be tested is mounted at the center of the sphere. The light reflected from the sample normally is focused by a cylindrical lens on the slit of a spectrometer.

The spectrometer is of the constant deviation type, and is especially designed for high intensity. Both entrance and exit slits are automatically variable, the former in lengt\ to compensate for lack of uniformity in the color sensitivity of the photoelectric cell, the latter in width to keep a predetermined width of wave-length band on the photoelectric cell.

The amplifier is as described in the previous paper. It is operated from an ac rectifier unit especially designed for it. This makes it possible to operate the analyzer from any 110 volt ac outlet.

The galvanometer record is photographed, on paper or film as desired, $3\frac{1}{4}$ by $4\frac{1}{4}$ inches in size, suitable for projection. All moving parts are driven directly from the film holder. Coordinates are printed in a separate compartment. An ac voltmeter records the voltage applied to the illumination unit on the same sheet with the reflection factor curve. Both the galvanometer and the voltmeter beams are reflected to a scale for visual observations.

It is expected to demonstrate the present model at the meeting.

Bibliography:
O. S. A. Colorimetry Com., J.O.S.A. & R.S.I., *6*, p. 527; 1922.
A. C. Hardy, J.O.S.A. & R.S.I., *18*, p. 96; 1929.

Saturday, October 26
3:00 P.M.

CONTRIBUTED PAPERS ON COLORIMETRY

(Continued from morning session)

Deane B. Judd **Bureau of Standards**

THOMAS YOUNG'S THEORY OF COLOR VISION AND THE HUE CHANGE BY ADDITION OF WHITE LIGHT

When both halves of a photometric field illuminated by spectral light of the same wave length and intensity, and white light is added to one side of the field, a normal observer will respond (for all but two wave-length regions) that the brighter and paler half of the field is no longer the same hue as the other half. This hue change was first reported in detail by Abney[1] and his results were subsequently confirmed by extensive observation at the Bureau of Standards.[2] Schrödinger[3] ascribed the hue change for red and green to the relatively high saturation of these colors in the spectrum compared to spectrum yellow, and this explanation was suggested independently by me[2] seven years later. The hue change for violet had not yet been explained.

The present work has shown that the three-components theory of color vision will qualitatively reproduce the experimentally established hue change for all wave lengths if the assumption (independently suggested by many other visual experiments) be made that the component for "blue" is less stable to light than that for "red" or "green." This assumption leads to the prediction that the two spectral regions showing no hue change shall be complementary (blue and yellow). We are further constrained to expect that (very pale) red may be produced by violet light merely on the addition of the proper quantity of white light. Both of these predictions have been verified by experiment.

Quantitative prediction has been made possible by the assumption of a specific form of variation with time, t, of the activity of the "blue" component: Activity $= K/(K+t)$, where K is a constant depending on the energy distribution of the stimulus. It has been found desirable to adopt for this purpose a set of primary colors differing slightly from the König Grundempfindungen. The findings of the Young theory extended in this way agree with experiment only semi-quantitatively, but the most serious discrepancies can be referred to the variation in saturation of spectrum colors already noted.

Bibliography:
[1] Abney, On the Change of Hue of Spectrum Colors by Dilution with White Light, Proc. Roy. Soc., A., *83*; 1909.
[2] Judd, The Empiric Relation between Dominant Wave Length and Purity, J.O.S.A. & R.S.I., *14*, p. 475; 1927.
[3] Schrödinger, Ann. d. Physik, (4), *63*, p. 514; 1920. Section 9, Die Farbtonänderung beim Zumischen von Weiss.

Irwin G. Priest and J. O. Riley **Bureau of Standards**

THE SELECTIVE REFLECTANCE OF MAGNESIUM OXIDE

The following data pertain to *one* sample of MgO deposited on aluminum by burning magnesium in air, the surface on which the oxide was deposited being 8 to 12 cm from the burning metal. The observed surface was deposited Aug. 22, 1929 on top of a similar deposit already 0.4 to 0.45 mm thick prepared several weeks earlier. The total final thickness was

0.54 to 0.64 mm. The deposit was quite compact, and the grain was very fine (showing hardly any recognizable structure for linear magnifications of over 100). It was apparently a less perfect diffusor than the common $MgCO_3$ blocks. Spectrographic analysis (B. S. spectroscopy section) showed absence of all metals except Mg, Ca (<0.005%), and possibly Fe in a very doubtful trace (perhaps 0.001%).

Measurements of reflectance were made by a *modified form* of Karrer sphere[2] with which one half of a Martens photometer field is illuminated by light from the sphere wall while the other half is illuminated by light from the sample which itself receives light *only* from the sphere wall.

R is apparent reflectance, on absolute scale, for line of sight normal to sample surface with illumination completely diffuse. R_d is R for daylight. R_b, R_g, and R_r are, resp., R for light transmitted by very selective blue, green, and red filters (Jena glasses 3654, 4930, and 4512) such that the spectral centroids of light are about 460, 540, and 650 mμ. R_m is the mean of R_b, R_g, and R_r. Values of R_d (for the purpose of describing the sample—not as important new data per se) as follows have been found (by comparison with $MgCO_3$ block for which McNicholas[1] had previously determined $R=0.981$):—Initial 0.4 mm layer, 0.97; the observed 0.6 mm layer, 0.984; a much thicker (>1.0 mm) layer, 0.987. For the 0.6 mm layer we have found:

$R_b/R_m = 1.0044$
$R_g/R_m = 1.0010$
$R_r/R_m = 0.9946$

Each of these values is derived from 8 determinations by I. G. P. and 4 determinations by J. O. R. (Sept. 8–18). Each of these 12 determinations comprised 10 individual measurements. The probable errors of the several "determinations" (computed from the 10 residuals in each case) range from about ± 0.001 to ± 0.002. Some of the measurements were made with the sample in one half of the photometric field, and others with it in the opposite half. Small instrumental corrections have been applied in both cases. In taking the means, equal weights were given to the two observers and to the two halves of the field. The probable errors of the means given are estimated to be less than ± 0.001.

Similar data on other samples of MgO are given in the following abstract.

We conclude that, *for these samples* of MgO, the reflectance for blue and green is greater than for red; but it is highly improbable that the reflectance for any wave length between 460 and 650 departs from the mean by as much as 1%. The first part of this conclusion has been verified by a separate qualitative experiment showing that the color of MgO (even for layers>0.5 mm) by multiple reflection is certainly blue, green, or blue-green. Direct comparison of color by multiple reflection with color by single reflection. (This is in direct contradiction of Pfund's finding[3] that MgO in thicknesses greater than 0.36 mm is yellow.) (See also the following abstract.)

Bibliography:

[1] McNicholas, B. S. Res. Pap. No. 3, p. 53.

[2] Karrer, B.S. Sci.: Pap. 415, Fig. 3. In the present work, illumination was provided by four small lamps *inside* the sphere, the sample being shaded from direct light from the lamps.

[3] Pfund, Jo. Frank. Inst., March. p. 379; 1920.

Irwin G. Priest **Bureau of Standards**

NOTE ON THE YELLOWNESS OF COMMERICAL MAGNESIUM CARBONATE AND THE ALLEGED YELLOWNESS OF MAGNESIUM OXIDE

For many years magnesium carbonate in the common form of blocks has been widely used as a standard white material in colorimetric work. However, it has been pointed out that its reflectance in the visible spectrum is actually not uniform but varies with wave length in such a way as to tend to make its color slightly yellow.[1] The yellowness of the sample dealt with in the present report is perceptible, by artificial daylight, in a 6° Martens photo-

meter field one half of which is illuminated by light from the wall of a hollow sphere while the other half is illuminated by light from the sample which itself receives light only from the same sphere wall. Measurements similar to those in the foregoing abstract (but less carefully made) show that the reflectance for blue is less than for red and green but, again, the departure from the mean between wave lengths 460 and 650 $m\mu$ is less than 1%.

Chemical analysis (B. S. chemistry division) showed 0.02% Fe in this sample. Spectrographic analysis (B. S. spectroscopy section) indicated the absence of all other foreign elements except Ca (<0.05%) and Cu (<0.01%). The well known color of iron oxides suggests at once the tentative hypothesis that the yellowness of common magnesium carbonate may be due to iron present as a very slight impurity. (To illustrate this, the speaker showed samples of zinc oxide paste containing ferric oxide in such concentration that the net iron content in the paste was only about 0.016%. When such mixtures were compared with the pure zinc oxide paste in daylight, the tinting produced by this small content of iron was easily perceptible.)

It was thought not inconceivable that the yellowness of MgO reported by Pfund might have been due to the same cause. However, the results of depositing MgO from magnesium contaminated by iron do not support this hypothesis provided that the oxide is deposited about 10 cm above the burning metal. Under this condition the iron content of MgO deposited by burning metallic magnesium is vanishingly small compared to the iron content of the metal burned, and seemingly independent of it. MgO deposited by burning Mg containing 0.09% Fe (by gravimetric analysis) showed (by spectrographic analysis) no more iron than that deposited by burning Mg containing only 0.006% Fe (by gravimetric analysis). In both cases the spectrographic analysis showed that Fe was probably absent or its presence very doubtful (perhaps 0.001%). Also, the presence of iron could not be established by spectrographic analysis of MgO which had been deposited by burning metallic magnesium which had been grossly contaminated by iron by rubbing and grinding finely divided (hydrogen-reduced) iron into it.

The oxide dealt with in the foregoing abstract was deposited by burning magnesium containing 0.006% iron. The data on selective reflectance for it are compared in the following table with like data for two other samples of oxide made by burning magnesium containing more iron.[2] These samples were (by spectrographic analysis), to all intents and purposes, as pure as the sample reported in the foregoing abstract. In all three cases spectrographic analysis of the oxide showed either that iron was absent or present only in a very doubtful trace (about 0.001%).

Fe content in magnesium	R_b/R_m	R_g/R_m	R_r/R_m
0.006% (foregoing abstract)	1.0044	1.0010	0.9946
0.02% (commercial ribbon)	1.0045	1.0007	0.9947
Grossly contaminated by rubbing and grinding finely divided iron into the magnesium	1.0036	1.0024	0.9940

Another possible hypothesis to account for the alleged yellowness of the MgO is the assumption that a small amount of magnesium nitride might be deposited with the oxide. The nitride is notably yellow. It is formed when magnesium is burned in air with insufficient draft to supply an adequate amount of oxygen; and is actually commonly found in the clinker residue resulting from the combustion of magnesium in air.

However, the definitive resolution of the discrepancy between Pfund's findings and our own must await the results of further investigation.

Bibliography:

[1] Hyde, Astrop. Jour., May, p. 239; 1912. Pfund, Jo. Frank. Inst., March, p. 379; 1920.

[2] For meaning of symbols in this abstract consult preceding abstract by Priest and Riley.

[In discussion of the above paper, at the meeting, Dr. Pfund stated that his *yellow* samples of MgO had been deposited within about ¼ inch of the burning magnesium, and that, in repeating the experiment, after reading the advance abstract of this paper, he had invariably obtained *blue* deposits, even for thick layers, when the deposit was made about 5 or 6 inches from the burning metal.]

Irwin G. Priest **Bureau of Standards**

NOTE ON THE RELATIVE SENSITIVENESS OF DIRECT COLOR COMPARISON AND SPECTROPHOTOMETRIC MEASUREMENTS IN DETECTING SLIGHT DIFFERENCES IN THE SPECTRAL DISTRIBUTION OF LIGHT

The following question (in effect) has been frequently put: Can spectrophotometric measurements be made with sufficient accuracy to detect and certainly establish the least differences which are perceptible by direct comparison of colors *as colors*? The question has been considered as particularly pertinent in the case of very pale tints hardly distinguishable from white.

The present answer is in the affirmative for particular cases, but it is not affirmed that this answer is general for all cases. Data on the selective reflectance of MgO and $MgCO_3$ are given in the two preceding papers on this program. Additional data by the well known psychophysical method of right and wrong answers in the direct comparison of the two halves of the 6° field in the Martens photometer with high illumination, one half of the field being illuminated by artificial daylight which has suffered *one* reflection from the sample while the other half is illuminated by light of the quality incident on the sample (see foregoing abstract), justify the following conclusions:

(1) The slight selectivity of the MgO, although definitely measured photometrically (by a single reflection), is entirely inappreciable by color discrimination under the conditions of observation just specified. (Greatly increasing the field size would tend to enhance the sensitivity but it seems highly improbable that any conditions of viewing would render this difference discriminable with certainty although it might attain the level of a doubtful vanishingly small difference.)

(2) In the case of $MgCO_3$ the perceptibility approaches but does not quite attain certainty. The observer reports the difference correctly in nearly all but not all trials.

Other similar data are also in accord with these conclusions.

On the other hand it should be emphasized that, in many cases, photometric accuracy must be pushed to the extreme possible limit in order to yield data truly significant in specifying color stimuli.

Bibliography:

Pfund, "The Colorimetry of Nearly-White Surfaces," Jo, Frank. Inst., pp. 371–379; March, 1920.

Pfund, "A New Colorimeter for White Pigments . . . .," A.S.T.M. 23rd Annual Meeting, June, 1920.

Priest and Riley, "The Selective Reflectance of Magnesium Oxide," this program.

Priest, "Note on the Yellowness of Magnesium Carbonate . . . .," this program.

PRESENTATION OF THE FREDERIC IVES MEDAL

Awarded by The Optical Society of America to Edward L. Nichols, Honorary Member of the Society and Professor of Physics, Emeritus, at Cornell University, for distinguished work in Optics.

This medal, awarded biennially, was endowed in 1928 by Herbert E. Ives in honor of his father, Frederic Ives, distinguished for his pioneer contributions to color photography, photograving, three-color process printing, and other branches of applied optics.

DR. EDWARD LEAMINGTON NICHOLS

PROFESSOR EDWARD LEAMINGTON NICHOLS

IVES MEDALLIST FOR 1929

At its recent meeting at Cornell University, the Optical Society of America awarded to Professor Edward L. Nichols, Professor Emeritus of Physics at Cornell, the Frederic Ives medal established by Dr. Herbert E. Ives in 1928 in honor of his father. The medal is awarded biennially for distinguished work in optics and it is particularly fitting that the jury of award for the biennium 1928–29, should have made the first award of the medal to Professor Nichols, who is one of the five honorary members of the Optical Society of America and whose contributions to optics in its various branches have covered a period in excess of a half century.

The presentation took place at a dinner arranged in Professor Nichols's honor by the Optical Society of America and his colleagues on the Faculty of Cornell University. Nearly two hundred guests attended the dinner.

Tributes of honor and respect were paid to the distinguished guest of the evening by Professor Simon Henry Gage, Professor Emeritus of Histology and Embryology at Cornell University, who acted as Toastmaster; Livingston Farrand, President of Cornell University; Dexter S. Kimball, Dean of the College of Engineering, Cornell University; Herbert E. Ives, donor of the Frederic Ives medal; Henry Crew, Professor of Physics at Northwestern University. Music was furnished by Mrs. L. L. Barnes and Professor V. Karapetoff.

In announcing the jury's award and presenting the medal, the President of the Optical Society called attention to the fact that the sole condition attached to the award by the terms of the endowment of the medal was that it be awarded "for distinguished work in optics." He exhibited the bibliography of Professor Nichols's papers, extending over a period of more than a half century, which had been prepared by the jury on award, and remarked that, although this bibliography itself fully justified the award, a mere list of Professor Nichols's publications, extensive as that list is, is by no means an indication of the full measure of Professor Nichols's life-long service to optics, for we must also remember that his well known work as a teacher and as a leader of research and the inspiration which he had given to many generations of students, both graduate and undergraduate, although less susceptible of reduction to a tangible record, were of vast and far reaching importance to the progress of science.

Returning to the bibliography, President Priest noted that it listed

year by year papers on such subjects as luminescence, physiological optics, photometry, and illumination, and that during a half-century there appeared to be hardly a year not marked by the publication of several papers.

The award of the medal to Professor Nichols makes it opportune to record briefly in the official Journal of the Optical Society of America a record of Professor Nichols's activities and achievements. He attended Cornell University during the period 1871–1875, receiving in 1875 the degree, B.S. Immediately thereafter he took up graduate work in Germany where he studied in turn at Leipzig, Berlin and Göttingen, receiving the doctorate from the latter University in 1879. Upon his return to America, he was a Fellow at Hopkins University in 1879–1880, and in that connection made contacts with the late Professor H. A. Rowland.

For a short time after his fellowship at Hopkins, he was associated with Edison in the development of the incandescent lamp. Professor Nichols, in this capacity, set up the first commercial photometer for measuring the candle power of incandescent lamps and developed the foundations of a technique which has since become of universal importance in all branches of photometric work.

After serving as Professor of Physics successively at the University of Kentucky and at the University of Kansas, Professor Nichols was called to Cornell to take the post of Professor of Physics in 1887. He became Professor Emeritus in 1919.

In recounting the achievements of this distinguished educator, scientist and friend, one is apt to lay emphasis upon those things, which, as President Priest said in his presentation speech, can be reduced to tangible form, such as for example, a bibliography of published papers. This bibliography is attached hereto and is, to be sure, in its entirety a most remarkable index of scientific activity covering a period of more than fifty years. When one remembers that Professor Nichols became Professor Emeritus in 1919, after a period of thirty-two years of activity on the Faculty of Cornell University, and further when one notes from the bibliography the number of papers which have appeared *since* 1919, one cannot but be impressed by the devotion of this man of science to the field of research to which he has given his life. In the decade since his retirement, Professor Nichols's publications clearly indicate that the retirement was, so far as research was concerned, a mere matter of form. Those of us who have had the rare privilege of being associated with him, particularly during this period, have received an inspiration which is not easily recorded.

During the thirty-two years of his directorship of the Department of Physics at Cornell University, Professor Nichols came in contact directly or indirectly with a large number of graduate students and instructors who now hold positions of responsibility throughout the entire country. The great majority of these have retained their interest in and devotion to research in physics, in many cases along the lines to which Professor Nichols, himself, has devoted so much attention.

Important as are Professor Nichols's immediate contributions to science, as is noted by the bibliography of his papers, and valuable as has been the inspiration which he has given, both over the lecture table and in the research room, to so many different students, both graduate and undergraduate, of still greater value is the influence which he has exerted by his insistence upon research as one of the prime functions, even duties, of the members of a university faculty. When he entered university life in the '80's, research was all but unknown in America. During his contact with the various research laboratories in Europe and later with that pioneer in physics, Professor Henry A. Rowland, at Hopkins, Professor Nichols came to appreciate the very great value of productive scholarship. Throughout his entire career, both by example and precept, he has consistently urged the importance of placing research in our American universities on a par with teaching; both should be regarded as *duties* of a university professor. It is comparatively easy for us now to preach this doctrine. The governing boards of most of the larger universities of America recognize its importance and take such practical steps as may be at their disposal to develop strong research centers in the respective institutions. But in 1887 it required a bold and far-seeing man to hold that universities should not only permit, but should expect, members of the faculty to do research—and a still bolder man to do as Professor Nichols did: *practice* what he preached! At that time, the main function of the professor was teaching. To maintain that he could devote a part of his time to research, was in some quarters almost regarded as heresy. When those of us who belong to the next generation following Professor Nichols compare the conditions of to-day with those of fifty years ago, we realize how much we owe to that comparatively small group of men in university circles, in which group Professor Nichols was ever one of the leaders, who insisted that research should be given a fundamental place in our American institutions of learning. What a satisfaction it must be to Professor Nichols to see his ideals so whole-heartedly adopted throughout America!

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THE DEFLECTION OF LIGHT AS OBSERVED AT TOTAL SOLAR ECLIPSES

By Charles Lane Poor

SUMMARY

The mathematical formula, by which Einstein calculated his predicted deflection of 1".75 for light rays passing the edge of the sun, is a well known and simple formula of physical optics. In this formula he substituted an hypothetical "retardation" of light in its passage through a gravitational field; and this purely theoretical retardation is the sole new concept involved in the prediction. Not a single one of the fundamental concepts of varying time, of warped or twisted space, of simultaneity, or of the relativity of motion is in any way involved in Einstein's prediction of, or formulas for the deflection of light. The many and elaborate eclipse expeditions have, therefore, been given a fictitious importance. Their results can neither prove, nor disprove the relativity theory: at the best their results can prove that light is retarded by gravitational action, and is retarded by a certain definite amount.

But the actual results of the eclipse expeditions do not even prove this. Not a single expedition, so far reporting, has made use of effective checks or controls for eliminating the effects of temperature upon their instruments, or for determining the possible effects of abnormal atmospheric conditions during the eclipse. And further not a single party has made a systematic study of all the data obtained. In the South American eclipse of 1919 less than 15% of the actual measured data was used in obtaining the announced results: in the Australian eclipse of 1922 only about 50% of the data was used. All non-radial components of the actual measures were discarded as "accidental errors." The radial components were then forced into a semblance of "exact accord with the requirements of the Einstein theory" through an assumption written into the methods and formulas of reduction: an assumption for which there is not the slightest observational evidence. In fact the only evidence available—the evidence furnished by independent check fields photographed on the eclipse plates—would seem to show that this basic assumption is utterly wrong.

The actual stellar displacements, if real, do not show the slightest resemblance to the predicted Einstein deflections: they do not agree in direction, in size, or in the rate of decrease with distance from the sun.

GENERAL STATEMENT

In 1911 Einstein[1] revived the suggestion of Sir Isaac Newton that "Bodies act upon Light at a Distance," and predicted that the gravitational action of the sun would deflect a ray, passing near its edge, by some 0".83 seconds of arc. General interest was aroused by his pointing out a definite method of experimentally testing this claim, or prediction. This proposed test involved the use of stars visible near the sun during

* Text of a paper presented at the annual meeting of the Optical Society, Ithaca, N. Y., October 25, 1929, as part of a Symposium on "Experimental Data Bearing on the Theory of Relativity." In publishing this paper, the Optical Society wishes to take no part in the arguments pro and con relative to Einstein's Theory.—Ed.

[1] Einstein: *Über den Einfluss der Schwerkraft auf die Ausbreitung des Lichtes.* Annalen der Physik, *35*; 1911.

the few moments of a total solar eclipse, and resulted in the first one of the now many celebrated eclipse expeditions. This first expedition was planned to observe the 1914 eclipse in Russia; but unfortunately the War intervened.

In 1916 Einstein[2] revised his theory and his calculations and stated that the deflection should be 1″.70 seconds of arc. But neither in his mathematical paper, nor in his popular works on Relativity, has he ever explained clearly and simply the reasons for this changed prediction, nor has he ever specifically stated the exact postulate, or postulates of relativity involved in his idea of bent light. All of these theoretical questions have been left in a state of confusion, with many contradictory claims and assertions. In his popular exposition of the subject Einstein in 1920[3] appears to claim that part of the expected deflection is caused by the "gravitational attraction" of the sun, and part by warped and twisted space. Eddington in 1920 states[4] that the entire deflection is due to the "weight of light"; and Trumpler[5] in 1923 specifically claims that the deflection is entirely due to gravitation, but that gravitation follows "Einstein's law instead of Newton's" The reports of the many eclipse expeditions do not help this situation. The astronomers in charge of the various parties do not seem to have investigated this theoretical side of the matter, nor to have made any attempt to discover what their costly expeditions might or might not prove as to the law of gravitation, the effect of gravitation upon light, or as to the many concepts and assumptions of relativity.

All of this confusion may be cleared up by an examination and comparison of Einstein's original papers. This was done in 1926 by Poor,[6] who reprinted Einstein's formulas of 1911 and 1916 and showed the basic assumptions involved in the predicted deflection of light. That these particular formulas were actually used by Einstein in his calculations was confirmed by Trumpler[7] in 1929, who, however, called attention to a numerical error in Poor's paper. From these two papers it

[2] Einstein: *Die Grundlage der allgemeinen Relativitätstheorie.* Annalen der Physik, *49;* 1916.

[3] Einstein: *Relativity: the Special and General Theory.* Translated by R. W. Lawson, 1920. Appendix III, page 153.

[4] Eddington: *Space, Time, and Gravitation.* 1920.

[5] Trumpler: *Historical Note on the Problem of Light Deflection in the Sun's Gravitational Field.* Astronomical Society of the Pacific: XXXV, No. 206; August, 1923.

[6] Poor: *The Relativity of Deflection of Light.* Journal of the Royal Astronomical Society of Canada; July-August, 1927.

[7] Trumpler: *Relativity Deflection of Light*: Astronomical Society of the Pacific, *41*, No. 239; February, 1929.

is now clearly established that in Einstein's formulas and calculations there is no trace of non-Euclidean geometry, or curvature of space. The basic formula, used by Einstein, is the ordinary differential formula of physical optics for the rotation of a wave-front in terms of the change in the velocity of the wave. This formula depends upon and involves the wave theory for the transmission of light. This simple and every-day formula was used in connection with an assumed retardational effect of gravitation upon the actual speed of light in and through space. This assumed retardational effect is the sole assumption, or postulate involved in Einstein's prediction that rays of light will be deflected as they pass the eclipsed sun.

Einstein has given two different formulas for this hypothetical effect of gravitation upon the speed of light. The formula of 1911 made the speed of the ray depend solely upon its momentary distance from the sun, and made this speed decrease as the ray approached nearer and nearer the gravitational body. This supposed effect of gravitation upon light is, thus, the direct reverse of its known effect upon matter. Gravatation accelerates matter: according to Einstein, it retards light. A particle of matter falling towards the sun, falls faster and faster: a ray of light moving directly towards the sun travels, according to relativity, more and more slowly. At the center of the sun, the speed of the falling particle would be infinite, the speed of light, zero. In 1916 Einstein introduced an additional factor into his formula, and made the actual speed of the ray depend upon the direction in which it is traveling. The introduction of this direction factor is the sole difference between the assumptions of 1911 and 1916, and is the direct cause of changing the prediction from the original 0″.83 to the final "General Relativity" value of 1″.75.

In his "Report" in 1918 Eddington[8] introduced a new formula for this supposed retardation of light by gravitation. Under the plea that "it is inconvenient to have the velocity of light vary with direction" he substituted a formula containing the factor 2, and which made the retardation, in all directions, exactly double that of Einstein's original 1911 formula. This formula of Eddington, however, cannot be derived from Einstein's fundamental concepts, or assumptions, and, through its factor 2, it has caused endless confusion and discussion. Trumpler's 1929 paper clears up the situation and shows clearly that while the two

[8] Eddington: *Report on the Relativity Theory of Gravitation.* The Physical Society of London; 1918.

formulas, Einstein's and Eddington's, give identically the same results for the total deflection from infinity to infinity, yet the paths of the rays in the immediate vicinity of the sun are radically different. To an observer on the earth a ray of light from a distant star, passing the edge of the sun, would be deflected by the same amount, whether Eddington's or Einstein's formula be correct: but to an observer on an hypothetical planet in the outer fringes of the Corona the difference would be marked.

The many and elaborate eclipse expeditions, therefore, have been given a fictitious importance. Their results, even if they be "in exact accord with the requirements of the Einstein theory" cannot prove the relativity theory; they cannot even prove the correctness of Einstein's formula for light retardation. Not a single one of the fundamental concepts of varying times, of warped and twisted space, of simultaneity, or of the relativity of motion is in any way involved in Einstein's prediction of, or formulas for the deflection of light. The sole concept of Einstein in these measurements and formulas is that of an hypothetical "retardation" of light in its passage through a gravitational field. At the best these eclipse results can prove the fact of such retardation, and can prove that a ray is retarded by a certain definite amount. These eclipse results cannot show anything in regard to the cause of the retardation; they cannot distinguish between different theories and formulas, which give identically the same results for the observed deflections.

Now in pure theory the eclipse method, suggested by Einstein for detecting possible light deflections, is very simple: in practice it is extremely difficult and is beset by many sources of error. Two photographs are taken: one during the eclipse showing the stars in the immediate vicinity of the sun; the other, taken at night, showing the same group of stars as they appear unaffected by the presence of the sun. A comparison of such two photographs, it was hoped, would show the presence or absence of deflections, or displacements in the positions of the stars on the eclipse plates. But a direct comparison of the two plates is impossible, for the positions of the star images on both plates are affected by many instrumental and physical conditions: by conditions inherent in the photographic process, by instrumental defects, and by varying atmospheric conditions. Some idea of the instrumental difficulties involved may be gathered by the mere statement that the night photographs of the Lick Observatory Expedition were taken in May at Tahiti; that the instruments were dismounted, transported

to Western Australia and the eclipse photographs taken in the latter part of September.

THE PHOTOGRAPHIC METHOD

The fundamental property of a photograph of a star group, or portion of the heavens is, that it is a projection of the celestial sphere from the optical center of the lens upon the plane of the photographic film. Any great circle in the sphere is projected into a straight line on the plate; and any straight line on the plate corresponds to a great circle of the celestial sphere. This projection is not mathematically exact, for the lens, camera, and plate are all subject to defects inherent in all physical apparatus; and the position of each stellar image is affected by atmospheric refraction and aberration. Omitting for a moment these purely astronomical factors, the problem, which faces an astronomer, is that of converting straight line measures on the photographic plate into their corresponding equivalents in the heavens. It is a comparatively simple matter to put the plate into a measuring machine, equipped with microscopes and micrometer scales, and to measure the rectangular coördinates of each image in terms of millimeters and in reference to some arbitrary origin, or center. But it is by no means a simple matter to transform these plate measures into the corresponding celestial coördinates and to find the exact positions of the stars in the heavens.

The optical axis of the lens furnishes the reference points both on the plate and in the heavens. The point in the heavens towards which this axis is directed is the origin to which all stellar measures must be reduced; while the center of the plate is the point on the film cut by a perpendicular from the optical center. If the plate be exactly perpendicular to the axis of the lens, then the center of the plate directly corresponds to the standard point of reference in the heavens. If not, an instrumental error is introduced into all the measures. Further this center of the plate is entirely unmarked and unknown; the actual measures being made in reference to some arbitrary zero point. These plate measures must, therefore, be corrected for this difference between the arbitrary zero point and the true center of the plate, and for all instrumental errors, and errors of plate adjustment, both in the telescope and in the measuring machine. These corrections are made through the use of "plate constants," so called because they are the same, or constant for a given plate and for a given set of measures of that plate. And after these corrections are made, the millimeter measures of the plate must be transformed into the equivalent angular measurement (degrees, minutes, and seconds) of the heavens.

This last transformation requires a knowledge of the most important of all plate constants. This is the scale of the plate, or the number of seconds of arc in the heavens represented by one (1) millimeter on the plate. This scale depends upon the exact distance of the surface of the plate from the optical center of the lens. While this distance is approximately known, it can never be directly measured or determined. The focal length of a lens varies, and is extremely sensitive to temperature differences. The tube of the telescope expands and contracts, and no two plate holders can be made exactly alike. Further no two plates are of the same thickness, so that the surfaces of two plates, exposed within a few moments of each other, may be at different distances from the optical center of the lens; and, thus, the scales of the two plates may be different. In cameras, such as were used by many of the eclipse expeditions, a change in focal length of only one-half (0.5) millimeter would change the scale sufficiently to give a star three (3°) degrees from the center of the plate an apparent displacement of nearly one (1″.0) second of arc. Direct measurements of focal lengths cannot be made with this necessary degree of accuracy, and so, there is no possible way of determining the scale of a plate except from the star images upon it.

This holds true for all the plate constants: they must be found from the star images on the plate itself. The true, rectangular coördinates (X, Y) of any star, as expressed in arc, are connected with the measured plate coördinates (x, y) by linear equations of the general form:

$$X = a + bx + cy$$
$$Y = a' + b'x + c'y$$

where a, b, c, and a', b', c' are the various plate constants, including the scale value. In general there are six constants, as shown; but this number may vary with the requirements of the particular problem. Now, if there are three stars on the plate whose positions in the heavens are accurately known, then for these three stars one knows, or can calculate accurately the values of the measured coördinates (x, y) and of the standard coördinates (X, Y). When these special values are substituted in the above general equations, one then has six simple linear equations involving the six unknown constants. From these six equations the values of the six constants (a, b, c, a', b', c') can be found by simple algebraic processes. If more than three stars are accurately known, values of the constants can be found from the resulting eight, ten, twenty, or more similar equations by the method of least squares.

The use of many known stars in this manner eliminates accidental errors of measurement, and gives the best, or most probable values for the plate constants.

When extreme accuracy is required and all possible sources of error are to be eliminated, then more plate constants must be used. This is especially the case where wide-angled lenses are used and distances from the optical center become relatively great. For in such cases terms of the second and third orders may be appreciable. The use of six and eight constants is usual. In some of the reductions of the Lick Observatory plates taken with the 15-foot cameras twelve (12) constants were used; and in the corresponding reductions with the wide-angled 5-foot cameras twenty (20) constants were used. In such cases the labor of making the least square solutions becomes enormous.

When for any plate these constants, 4, 10, or 20, have been determined from measurements of known stars, then they may be used to find, from the measured coördinates, the exact position of some unknown body, or of a star whose position had never before been accurately determined. It should be perfectly clear, however, that these "plate constants" cannot be found from stars whose positions are not accurately known, or from stars which are subject to some unknown and undetermined displacement. It is this very difficulty which has confronted the various eclipse parties. The stars photographed during the eclipse cannot legitimately be used to determine these plate constants, especially the scale value. For it is the very essence of the problem that the positions of these stars are "unknown." This fact is now, many years after the eclipse photographs were taken, clearly recognized, and has been admitted by Trumpler in these words:[9]

> The scale of the eclipse field photographs is necessarily linked together with the light deflections; the two cannot be separated unless some assumption is made concerning the law to which the light deflections are related to the star's angular distance from the sun's center.

Various methods have been suggested for overcoming this fundamental difficulty, but no satisfactory method has yet been used by the various eclipse parties. The methods actually used in determining these constants will be explained in the discussion of the results of the different parties. And upon the validity of such methods depends the value of their announced results.

The standard method, heretofore used for all photographic work,

[9] Trumpler: "Observations made with a Pair of Five-Foot Cameras on the Light-Deflections in the Sun's Gravitational Field at the Total Solar Eclipse of September 21, 1922," Lick Observatory Bulletin, No. 397; 1928, page 137.

would thus require the measuring of each plate separately, the determination of the plate constants, and then the calculation of the standard celestial coördinates of each star. Such coördinates of each star, as determined from the night plate, could then be compared with the similar coördinates as shown by the eclipse plate; and the displacement, or deflection directly determined. This regular, standard method, however, would require an immense amount of labor. Not only would each plate have to be separately measured, but each plate would require long, tedious least square solutions. In order to simplify and shorten this work, the English astronomers, in 1919, devised a method of measuring and reducing their photographs by indirectly superimposing one plate upon the other, and by measuring the differences in positions of the corresponding images of the same star on the night and eclipse plates. This indirect method involved the use of a special photograph, *R*, of the star group taken at night with the plate reversed, so that the light passed through the glass of the plate before reaching the film.

The two regular negatives, the night plate, *N*, and the eclipse plate *E*, can only be superimposed film to glass. And, in this position with the thickness of the glass between the two films, the relative positions of the star images would be affected by the distortion effects of the glass plate. This difficulty was overcome by the use of the special plate, *R*. This plate could be placed in contact with any one of the regular night or eclipse plates, film to film; and when so placed the images of the same star on the two plates would appear very close together. The small distances between the corresponding images could be measured with extreme accuracy. The images on this special plate, of course, were not in their normal positions, for the light in passing through the glass would be bent from its normal course. But this was of no real importance, for this special plate merely provided a convenient point of reference near each star, to which could be referred the images on each and all the plates in turn.

The process was first to clamp the regular night plate, *N*, to the reference plate, *R*, in such a way that the pairs of images were reasonably close together all over the plate. The relative positions of the images, *NR*, in each pair were then measured; the measures being made in the *X* and *Y* coördinates. After these measures were completed, the reference plate, *R*, was clamped, film to film, to the regular eclipse plate, *E*, and again adjusted so that the new pairs of images were close together. The differences in position of these pairs, *ER*, were then measured. From these two sets of measures the relative position of the eclipse

image of any star to the corresponding night image could be then directly determined by subtraction, by the difference:

$$ER - NR$$

This indirect method gives with extreme accuracy the shift of each individual image from night plate to eclipse plate, but it does not give the actual coördinates of the various stars. But for the investigation of possible light deflections, the actual coördinates are not necessary; and this method gives all required data with a minimum of labor.

The results of such measures give the difference in coördinates, Δx and Δy, for each star in passing from the night plate to the eclipse plate. The result is the same as though the two plates had been superimposed and these differences measured directly. This is shown in the accompanying figure in which the displacements, however, are purely imaginary and are enlarged many thousands of times. The large dots represent the positions of a number of imaginary stars on the night plate:

FIG. 1. *The measured displacements. Through this indirect method the eclipse plate is practically superimposed on the night plate, and the difference in coördinates* (Δx, Δy) *of each star are measured. The large dots represent the stars on the night plate: the small dots the corresponding images on the eclipse plates. These measured differences are subject to many instrumental and theoretical corrections; and all such corrections must be applied before anything can be determined as to possible light deflections.*

the small dots represent the positions of the same stars on the eclipse plate, on which may also be seen the Corona extending out beyond the edge of the Moon. These differences, Δx and Δy, vary radically for the different stars on the plates, and they appear to vary in a more or less haphazard manner. This is due to the fact that the centers of the plates do not exactly coincide, and to the further fact that the images on the two plates are differently affected by many instrumental and theoretical errors and corrections.

Omitting the purely astronomical corrections for refraction, aberra-

tion, proper motion and stellar parallax, these measured differences, $\Delta x, \Delta y$, between the eclipse and night plates are affected by the following instrumental errors inherent in the photographic method:

a. Scale differences of the two plates. The night and eclipse plates are taken at different localities, at different seasons of the year, and under radically different atmospheric conditions. The "scales" of the two plates will most certainly be different, and this difference of scales will affect the measured displacement of each star. All the images on one plate will appear radially displaced in reference to the corresponding images on the other plate. And the more the scales differ, the greater will appear this radial shift.

b. Different inclinations of the two plates to the optical axis of the telescope.

c. Optical distortions of the lens system. These distortions may be quite large for the wide-angled lenses used by the eclipse parties. While they depend mainly upon the position of the individual star in the field, and thus tend to be the same for both plates, yet they also depend upon various instrumental adjustments of lens and plate, and may thus cause a residual effect upon the measures.

d. Inaccurate centering of the two plates during measurement. The centers of the two plates are unmarked and unknown, and in the process of clamping these plates to the reference plate there will be introduced an unknown offset between the two centers. The effect of this offset is the same for all the stars.

e. Inaccurate orientation of the two plates during measurement. In the process of clamping the two plates to the reference plate, it will always happen that one is twisted a little in reference to the other. This twist throws all the images of one plate to the right or to the left of those on the other, but it affects the different stars in varying amounts. The shift is proportional to the distance of the image from the center of the plate: the outer images are shifted the most; the inner ones the least.

Now all these instrumental errors and effects due to the method of measuring the plates must be calculated for each star, and the measured Δx and Δy corrected for such errors and effects before any study can be made as to whether or not these images indicate anything in regard to possible light deflections. These corrections all depend upon the so-called plate constants, or more correctly upon the "differences" of the plate constants. These constants, it has been pointed out, cannot be found by any independent calculations or measures: they can only be

derived from stellar images of known stars on the plates themselves. And similarly, the "differences" of the constants, which are necessary for the reductions in this shortened method, can only be found from measured Δx's and Δy's. But it is self evident that the Δx's and Δy's used in the determination of these constants must themselves be accurately known, or be subject only to accidental errors of observation. These constants cannot be determined from the images of stars whose positions are unknown, or which are subject to some unknown and undetermined displacement. The stars of the eclipse field, the stars supposedly subject to the Einstein light deflections, cannot legitimately be used to determine these constants. To do so, is to write the Einstein deflection into the formulas of reduction, and to assume the existence of the very effect that the entire expedition was organized to search for.

ASTRONOMICAL CORRECTIONS AND SOURCES OF ERROR

In addition to the difficulties inherent in the photographic method of determining star positions and displacements, there are several astronomical complications in the eclipse method, which may introduce serious errors. The most important of these are:

1. Differences in the effect of atmospheric refraction on the two photographs.

The sought for stellar displacements are of the second order as regards the theoretical, or tabular corrections for ordinary atmospheric refraction. The effect of refraction, as is well known, is to raise the apparent position of every star; is to make each star appear nearer the zenith of the observer than it really is. The amount of this shift is greater for stars near the horizon than for those higher up. The stars on the photographic plates are at varying distances from the zenith, and their images, therefore, will be differently affected by this atmospheric refraction. The average displacement of all the stars on a plate of the Australian Eclipse Expedition, for example, was some 33″ of arc; and the differential refraction, or the difference between the refraction for the center of the plate and for any stellar image, was approximately 1″.30 of arc for each degree (1°) of distance from the center. That is, for a star 3° away from the sun and near the top or bottom of a plate taken with the 15-foot cameras of the expedition, the measured distance from the center would be about 4″.0 of arc too small; for a star 8° away from the sun, as photographed with the 5-foot cameras, this theoretical refraction correction would be over 10″ of arc. This is over one hundred (100) times the sought for Einstein displacement of such

stars. Thus an error on only one (1%) per cent in calculating the refraction correction would completely change the supposed displacement of the more distant stars.

From the way in which the photographic plates were measured and reduced, it is evident that each measured Δx and Δy must be corrected for the difference in refractive effects on the night and day plates. These refractions are calculated from standard tables, which have been developed through many years of exact astronomical observations. The amount of the refraction in any particular case, as given by these tables, depends not only upon the position of the star relative to the zenith, but upon the momentary conditions of the atmosphere as evidenced by the temperature and barometric pressure. The change in refraction with varying temperature is more important that that corresponding to barometric fluctuations. The refraction changes materially with the temperature of the atmosphere; increasing as the temperature falls. For stars 8° away from the center of an eclipse plate, this increase in the differential refraction would be about 0″.01 of arc for each 1°F drop in temperature. The use, therefore, in the tabular calculations of a temperature, which does not correctly represent the actual temperature of the free air at the moment of observation, may introduce a fictitious displacement of the stellar image directly comparable with the supposed Einstein deflections.

But a greater difficulty faces the astronomer. These theoretical refraction tables are based entirely upon stellar observations made during night time; upon observations selected to represent typical night conditions of standard temperatures and barometric pressures. And during the night time temperature distributions in the atmosphere are radically different form what they are during the day. The general average diminution of temperature with height near the ground, day and night, taking the whole year round is not far from 6°.5C per kilometer. During the day time this diminution is much more rapid; while at night it is much less, and may even, for small altitudes, be changed to an actual increase. Now the refraction is not greatly influenced by changes in the temperature of the atmosphere near the ground; but is dependent upon temperatures at considerable heights from the surface of the earth. These temperatures of the higher atmosphere cannot be directly measured at the time of each observation: they must be inferred from the temperatures measured on the surface of the earth. A ground temperature of 80° at midnight, however, represents quite a different temperature at several thousand feet altitude, than does the same 80° registered

on the same thermometer at midday. As the temperature corrections of all standard refraction tables are based upon night conditions, and upon the average night diminution of temperature with altitude, it is clear that these tables will not ordinarily give correct results for day observations.

Now in the eclipse method one of the photographs is taken at night, the other in the day time, and the refraction corrections for the same star, for the same zenith distance, and for the same readings of barometer and thermometer, may be and probably are quite different for the two photographs. Yet the corrections, as taken from the standard tables, would be identically the same. The use of tables, derived for night observations, for the reduction of day time observations introduced unknown errors, which may or may not be large enough to completely mask the sought for deflections. All of this has been known for years; but, as no standard tables have been derived for daylight observations, the various eclipse parties disregarded this great source of error, and reduced both their day and night plates by the same theoretical tables of refraction.

Possibly one of the clearest statements in regard to this difference of refractions between day and night is contained in a recent paper by R. H. Tucker of the Lick Observatory, who states:[10]

"There is a well established change in the vertical refraction between day and night, at this station, which amounts to 0".5 tan.Z.D. This change takes place near the epochs of sunset and sunrise."

This change is slightly more than one (1%) per cent of the tabular refraction; and, if correct, would introduce an error of practically 0''.05 of arc in the supposed positions of stars 3° away from the center of the eclipse plate. This is one-third (1/3) of the supposed Einstein deflection at this point.

2. Abnormal refractions in the earth's atmosphere due to the passage of the eclipse shadow.

As has been noted all tables of refraction give theoretical refractions for normal night distributions of temperature and pressures. These tables do not give correct results for ordinary day observations, even when the atmosphere is apparently normal. During the moments of a total solar eclipse, the atmospheric conditions are very abnormal. The heat of the sun is almost instantly withdrawn from the column of air over the observing station; the temperature drops suddenly, and atmos-

[10] Tucker: *Errors of Time Determination.* Astronomical Society of the Pacific; *37*, No. 219; October, 1925.

pheric disturbances and currents may be set up. Such abnormal conditions of the atmosphere may well give rise to abnormal refractions, to refractions in azimuth as well as to refractions in altitude. And such abnormalities can neither be computed or allowed for.

The effects of such abnormal refractions, if they exist, would be shown by the observed displacements of the stars on the eclipse plate in reference to those on the night plate. The deflections would not be radial; they would vary in direction for stars in different portions of the field, and the size and directions of such deflections should change with the passage of the shadow over the observering station: "exposures during the first part of totality should give different results from those taken at the middle or towards the end of totality."[11]

3. Differences in the effects of aberration and proper motion.

Aberration is a shift in the apparent position of the stars due to the motion of the earth, and such shift is, therefore, different at the different seasons of the year. This theoretical effect can be calculated by standard methods.

The actual positions of the stars in the heavens may change during the six months period which must elapse between the taking of the two photographs. This shift is the so-called proper motion of the stars, and, if it is known for a star, its effects can be calculated and allowed for.

4. The so-called "yearly," or Courvoisier refraction.

This is an apparent displacement of all stars away from the sun, which has been found by meridian observations. A general empirical formula has been developed which appears to give satisfactory results for stars situated at some distance from the sun; but no independent observations are available for testing the correctness of this formula in the immediate vicinity of the sun.

5. Possible refractions in the Corona and in an extended solar atmosphere.

Such refractions, if they exist, would be of a similar character to the predicted Einstein displacements, and would be practically indistinguishable therefrom. It has been assumed by relativists and by the astronomers of eclipse expeditions that the density of the Corona is so small as to make these refractive effects negligible.

CHECKS AND CONTROLS

Many practical suggestions have been made to eliminate, or to control some one or more of these sources of error. Such suggestions include

[11] Trumpler: Lick Observatory Bulletin, No 397, page 157.

the use of reséaus and check fields photographed on the eclipse plate to control film distortions, plate constants, and instrumental errors; include the use of very short exposures of the Moon on the eclipse plate to check abnormal refractions, and to determine the point on the path of the ray at which the observed displacements occur. The earlier expeditions did not use any of these checks or controls; and their results are of no real value. This applies especially to the well-known British expedition in 1919, the results of which were hailed as furnishing a complete proof of the entire Einstein theory. Not only were no controls used, but the instrumental equipment of this expedition was unsuitable, assymmetric, and easily deformed by unavoidable temperature changes. Instruments of the British type had been condemned as worthless for purposes of exact measurement in the reports of the transit of Venus expeditions of 1882.

Later expeditions made partial use of one or more of these possible checks, but not a single expedition, as yet reported, had a complete system of checks and controls. The Croker Eclipse Expedition of the Lick Observatory, the most famous of all such parties, made partial use of check fields to control the effects of film distortions; but had no effective temperature controls, no check or control for abnormal refractions, and determined the "scale value" directly from the eclipse stars themselves. The best planned expeditions were those of Swarthmore College, under Professor John A. Miller, to the eclipses of 1923 and 1925. Controls including check fields and the Moon were devised, but unfortunate accidents and weather conditions prevented success.

THE BRITISH EXPEDITION OF 1919

The printed Report[12] of the astronomers in charge of this expedition gives many interesting details as to the instruments, the methods of taking the photographs, of measuring the plates, and of reducing the results. The expedition was divided into two parties; one of which observed the eclipse at the island of Principe, off the west coast of Africa; the second party at Sobral, a small town some miles inland from the northeast coast of Brazil. The two parties were equipped with horizontal photographic telescopes: the Principe party having one such telescope with a lens 13 inches in diameter: the Sobral party having two cameras, a 13-inch lens and a 4-inch lens of 19 feet focus. This use of

[12] Sir F. W. Dyson and others: "A Determination of the Deflection of Light by the Sun's Gravitational Field, from Observations made at the Total Eclipse of May 29, 1919," Royal Astronomical Society: Memoirs, *62*, No. 9; 1919.

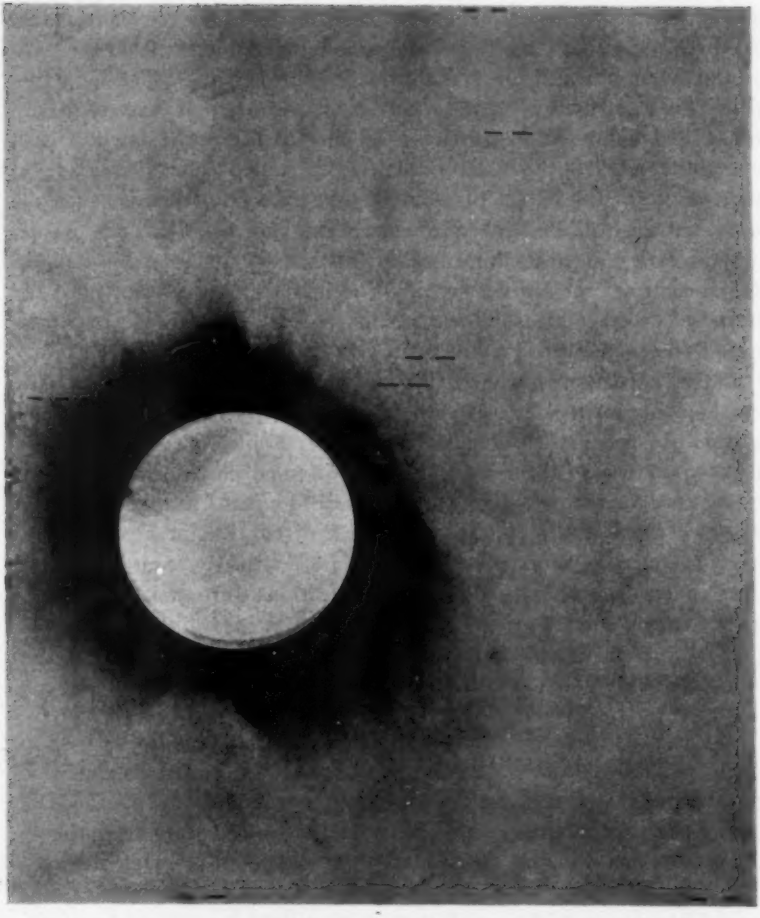

FIG. 2. *An eclipse photograph. Except for the dashes, which were put on to call attention to the minute star images, this is an untouched reproduction of the original negative taken at Sobral, Brazil, on May 29, 1919. The positions of these images were compared with similar images on plates taken on the nights of July 14-18. In making the comparisons the presence of the Einstein deflections were assumed, and the Einstein law of decrease with distance was written into the formulas.*

horizontal telescopes, with their plane mirrors, was an error which has practically invalidated the results of the entire expedition. The fact that such form of mounting is inadequate for exact measurements had been known for many years, and is amply evidenced by many statements in the Report itself. Trouble was experienced with the mounting and driving clocks of the mirrors during the setting up and the adjusting of the instruments; troubles so serious that "marked oscillations of the images" were apparent. And, under the action of the sun's heat, the shape of the mirror changed and threw the images out of focus. Many of the plates taken during the eclipse were, thus, rendered worthless, and doubt cast upon the others. When, immediately after the eclipse, some of the plates taken with the 13-inch camera were developed, the following note was made:

"It was found that there had been a serious change of focus so that, while the stars were shown, the definition was spoilt. This change of focus can only be attributed to the unequal expansion of the mirror through the Sun's heat."

With these three telescopes, or cameras the astronomers took forty-three (43) plates of the eclipsed sun and of the stars in its immediate vicinity; thirty-five plates (35) with the astrographic lenses of 13-inches aperture, and eight (8) plates with the auxiliary camera. Passing clouds interfered and only thirty (30) of these plates showed a sufficient number of star images to warrant measurement. After measuring these plates many more were discarded. The final statement in the Report mentions twenty-five (25) plates only, and sixteen of these are described as giving discordant results of little or no value. This leaves but nine (9) out of the original forty-three (43) plates, and of these remaining plates seven (7) were taken with the small auxiliary camera. The entire supposed confirmation of the Einstein deflection depends upon the measurements made upon these seven (7) selected plates. These seven plates gave an average deflection of $1''.98$: the discordant and discarded plates gave an average deflection of only $0''.93$.

No checks or controls of any kind were used. Notwithstanding the extreme sensitiveness of the instruments to temperature changes, apparently no precautions were taken to protect the instruments, or even to record the actual temperatures of the instruments at the moments the exposures were made. The tabulated list of temperatures in the Report was "communicated by Dr. Morize," acting on behalf of the Brazilian Government, and gives the temperatures at intervals of fifteen (15) minutes. Only one record seems to have been made during the entire period of totality. Apparently all refraction corrections were cal-

culated with this one standard temperature, for the same refraction constants were used for all the eclipse plates.

The Report, however, contains full and complete details of the methods used in measuring and reducing the seven plates of the 4-inch camera. The actual micrometer measures are given for each star, and essential steps, equations, and figures in the process of reduction are set out at length. In this respect this Report is a model, which unfortunately has not been followed by later expeditions. The measurements were made through the use of a reference plate, as heretofore explained. The plate constants, including the scale, were determined from the measured displacements of the eclipse stars by a method which presupposes and depends upon the existence of the Einstein effect. For measured values of Δx and Δy for each star, equations were formed of the general type:

$$\Delta x = ax + by + c + \alpha E_x$$
$$\Delta y = a'x + b'y + c' + \alpha E_y$$

where, a, b, c, a', b', and c' are the plate constants for centering, orientation, and scale value, as heretofore explained. The quantity α denotes the light deflection at unit distance and E_x and E_y are numerical coefficients, calculated by Einstein's formula, to turn the standard radial deflection, α, into the corresponding x and y components for each star. The equations, so formed, were solved by least squares, and the values of the plate constants and of α were found for each plate.

This method writes the Einstein law into the reduction, and makes the values of the plate constants depend upon the existence of the very light deflection, which the expedition was organized to search for. And further, even this method does not seem to have been consistently carried out. For the solution of the many equations seems to have given different scale values for the different plates. But the actual computation of the supposed deflections were carried out with "adopted values for the scale." How these adopted values were found is not stated: they are near the mean, or average values, but not quite. And we have seen that very slight changes in the scale value, may completely change the character of the supposed deflections.

It would be just as inaccurate to omit this Einstein term from the equations and to then find the constants from the remaining terms. For this is merely to write a different assumption into the reductions: the assumption that there are no deflections; or more accurately, that stars at the average distance from the sun have no deflections. There

is no possibility of obtaining the values of the plate constants from the eclipse stars, themselves, without some definite assumption as to the presence and character of possible stellar displacements. And it is a fundamental property of all least square solutions that, the greater the number of unknown quantities in the equations, the smaller will be the final residuals and the more nearly will the assumption, upon which the unknowns are based, appear to fit the observations. Thus the insertion of the unknown, α, into the equations naturally must make the final residuals smaller than had the equations been solved for the constants only. Similarly, if some possible assumption in regard to the deflections, based upon abnormal refractions, or instrumental distortions due to temperature differences and involving three unknowns, α, β, and γ, had been written into the equations, then, under definite, immutable laws of mathematics, the final residuals would be smaller than those obtained by the Einstein assumption. And a comparison of final results would make it appear that this assumption was the better, or more plausible.

The astronomers, thus, reduced their selected plates by writing the Einstein law into their formulas, and by this process of assuming the existence of the predicted deflections, they obtained the following final, or mean results for the plates taken with the three cameras:

"From the seven (7) plates taken at Sobral with the 4-inch camera, 1".98 with a probable error of about $\pm 0''.12$.

From the two (2) plates taken at Principe with the 13-inch astrographic lens, 1".61.

From the sixteen (16) plates taken at Sobral with the 13-inch astrographic lens, 0"93. "For reasons already described at length not much weight is attached to this determination."

These results, in each case, are the means of the radial components only; nothing whatever being given as to the directions in which the actual displacements took place. The Einstein theory requires a deflection, not only of a certain definite amount, but also in a certain definite direction. To discuss the amount of the observed deflection is to discuss only one-half of the whole question, and the less important half at that. The observed deflection might agree exactly with the predicted amount, but, if it were in the wrong direction, it would disprove, not prove Einstein's claims. You cannot reach Washington from New York by travelling north, even if you do go the requisite number of miles. An inspection of the Report shows that in the case of only one star was the deflection practically in the right direction: in all other cases the measured deflections differed *many* degrees from radiality. But the

British astronomers evidently regarded the question of direction as non-essential; regarded the transverse components as mere "accidental errors" of measurement.

While the average result for the radial displacement, as determined from the seven (7) plates, was somewhat in accord with the Einstein prediction, yet the displacements of the individual stars differed radically from the corresponding relativity amount. These differences range from 5% for the best star to well over 60%. Further the rate of decrease in size of the deflections with increasing distance from the sun does not agree at all with the Einstein law. According to theory the deflection of the star nearest the sun should be 2.75 times that of the most distant star; actually, according to the observations, this ratio was 5.10. Thus even the few selected observations do not agree with the Einstein prediction: do not agree in direction, in amount, or in the rate of decrease with distance from the sun.

When, however, the deflections from each plate are separately examined, then a curious and very important result appears. Plate No. 1 was taken at the beginning of totality; Plate No. 8 just before the eclipse ended. In the case of every star, the measured deflection is smaller on Plate No. 1 than on Plate No. 8, and in most cases this difference is extremely marked. This systematic change in the size of the deflection from plate to plate is clearly brought out by dividing the plates into three groups, and by taking the average deflection of all the stars on all the plates of each group. The average deflections, thus found, are:

Plates, Nos. 1 & 2	0″.30
Plates, Nos. 3, 4, & 5	0″.34
Plates, Nos. 7 & 8	0″.38

There thus appears to be an increase in the size of the deflection of some 27% as the eclipse progressed. And again, the directions of the observed shifts show corresponding changes with the progress of the shadow cone: there are marked differences between the first and last groups of plates in the cases of six out of the seven observed stars.

This change in the size and direction of the measured deflections as the eclipse progresses indicates some connection between the stellar deflections and the position of the shadow cone in the earth's atmosphere. This would appear to show that some portion, at least, of the measured deflection is caused by some effect of the earth's atmosphere; by some sort of abnormal refraction.

If the results of the British expedition prove anything, they prove that the observed deflections were not caused by the relativity effect. These results do not offer the slightest evidence to support the Einstein claims.

THE LICK OBSERVATORY EXPEDITION OF 1922

The Crocker Eclipse Expedition of the Lick Observatory was well organized. The equipment was specially designed for eclipse work, and the optical parts of the instruments were well adapted for obtaining the best photographic results. The details of the expedition have been published in two parts: in Lick Observatory Bulletins, No. 346 issued July 5, 1923; and No. 397 issued March, 1928. The first paper by Campbell and Trumpler, gives the results from four (4) plates taken with twin cameras of 15-feet focal length, the lenses being 5-inch photographic doublets. The second paper, by Trumpler alone,[13] gives the results obtained from six (6) plates taken with twin cameras of only 5-feet focal length, the lenses being 4-inch wide angle quadruplets. This latter paper also attempts to reconcile the varied results of all ten (10) plates, and to make a definite, final determination of the light deflection as observed in Australia, In doing this, Trumpler gives many details regarding methods of measuring the plates and reducing the results, which were omitted from, or only alluded in the original paper of 1923.

The first fact that appears in regard to all these supposed observations is the relative size of the reputed displacements and the stellar images on the plates. As the silver grains of the photographic film differ greatly in sensitivity, many of the star images did not represent accurately the central point of the star's image as formed on the plate. Very few of the recorded images were round; most of them, viewed under the microscope, were irregular in outline. And further, the minimum theoretical diameter of a perfect stellar images was $0''.9$ of arc for the 15-foot camera, and somewhat over $1''.0$ of arc for the 5-foot cameras. The smallest photographic images, obtained with the best lenses, are about ten times the theoretical minimum diameter. According to Professor George E. Hale, of the Mount Wilson Observatory, the diameter of a star image on a plate

[13] In a signed Foot-note, Bulletin No. 397, Campbell states, in regard to Trumpler's work, that: "the laborous measurements of the plates obtained with the 5-foot cameras, the elaborate computations based upon these measurements, and the extensive discussion of the final and general results were made at Mount Hamilton in the past two years exclusively by him; and he also prepared the manuscript and the illustrations for the printer."

"varies from several seconds of arc to about five or six tenths of a second, the smallest, I think, yet photographed. These minute images were obtained by Hubble under good atmospheric conditions with the 100-inch telescope on Mount Wilson."

Thus the stellar images on the eclipse plates were, at least, several seconds of arc in diameter. The average deflection of the 92 stars photographed with the 15-foot cameras, as determined by Campbell after elaborate calculations, was less than four-tenths (4/10) of a second, or only a very small fraction of the smallest image. The average mean radial displacement of the 145 stars, photographed with the 5-foot cameras, was only one-tenth (1/10) of a second, or about one one-hundredth (1/100) the size of the actual stellar images. Thus the reported deflections are second order effects only: being very minute fractions of the actual, irregular images on the photographic plates.

Even a casual study of these two papers brings out clearly the fact that no checks or controls were used to determine, or eliminate the effects of varying temperatures upon different portions of the instruments. The "scale" of a photograph depends upon the focal length of the lens, and is, therefore, quite sensitive to changes in temperature of the whole apparatus, camera, tube, and plate; and might be radically affected by differences in temperature between the steel of the camera tube and the glass of the enclosed plate. Each lens, each piece of glass has its own peculiarities; and changes in temperature will affect one lens in a manner quite different from another. Yet, as far as these papers indicate, neither Campbell nor Trumpler ever subjected these lenses and cameras to temperature tests. There is no indication that the actual temperatures of the various cameras were even taken at the time of the eclipse and at the time of the night exposures. The temperatures of the air in the telescope houses are given; and Trumpler, in the second paper, gives some theoretical calculations as to changes in scale value due to changes in temperature. But such calculations were based upon the average theoretical expansions of steel and glass, and not upon experimental tests of the actual instruments. These theoretical calculations, made five years after the eclipse, show that temperature changes as small as 2°C would so alter the focal length of the camera and so change the "scale" as to completely change the character of the supposed deflections.

Further, no checks or controls were used to determine, or to eliminate the effects of abnormal refractions in the earth's atmosphere: no efficient temperature controls were used by which even the ordinary tabular refractions could be accurately calculated. These refractions de-

pend upon temperature, and the use, in the calculations, of a temperature differing only a few degrees from the actual temperature of the air would introduce a fictitious displacement of the stellar images equal to, or greater than the sought for Einstein effect on the more distant stars. With the passage of the eclipse shadow, rapid changes in temperature would seem to be inevitable, and such changes would be accompanied by varying refractive effects. Yet, so far as these papers indicate, no attempt was made to detect such probable temperature changes. No statement is made as to the type of thermometer used, of its situation in regard to the instruments, or of its sensitivity to changes. One standard temperature, 79°.2F, was used in the reductions of all the eclipse plates. And, in making the refraction calculations, no distinction was made between night and day plates. The standard formulas, methods, and constants for night observations were used in the calculations for the eclipse plates. Yet it has been known for many years that such procedure is inaccurate: it is a well established fact that for the same altitude, the same barometric and thermometric readings, daylight refraction differs materially from that at night.[14]

But the great difficulty that faced Campbell and Trumpler in transforming the measured coördinates into star positions was that of determining the "plate constants," and especially the "scale value." These cannot legitimately be determined from the eclipse stars; for the positions of these stars are unknown. Now in planning operations Campbell decided to use a "check field" to control possible film distortions and to assist in determining the plate constants. This was an auxiliary star group, which followed the eclipse star field by some 6^h15^m, but at the same declination. This check field could thus be photographed at night, both at Tahiti and at Wallal; and at both epochs it was more than 90° distant from the sun. The stars of this field, thus, were absolutely unaffected by the hypothetical relativity deflection of light, by the daylight, or by the Courvoisier refraction. By photographing this field upon both the night and the eclipse plates, the desired controls would be secured. Each plate, therefore, was doubly exposed: exposed to the eclipse field of stars and exposed to the check field. In making these double exposures, the plates "were left undisturbed in the telescope" between exposures. Such check fields were photographed on all the plates taken with the 15-foot cameras; with the 5-foot cameras, four plates were so exposed to check fields, but two plates were not.

[14] Tucker: "Errors of Time Determination." Astronomical Society of the Pacific; 37, No. 219; October, 1925.

The use of such check fields on the eclipse plates probably afforded satisfactory controls against permanent distortions of the film; either in drying, or caused by the over-exposed central portions of the plate. The tabular results, given by Campbell and Trumpler, for these check fields on the eclipse plates indicate very small systematic distortions, probably not over 0".05 of arc. Such distortions, if real, are too small to have any bearing upon the Einstein problem, where displacements of several tenths of a second are sought for.

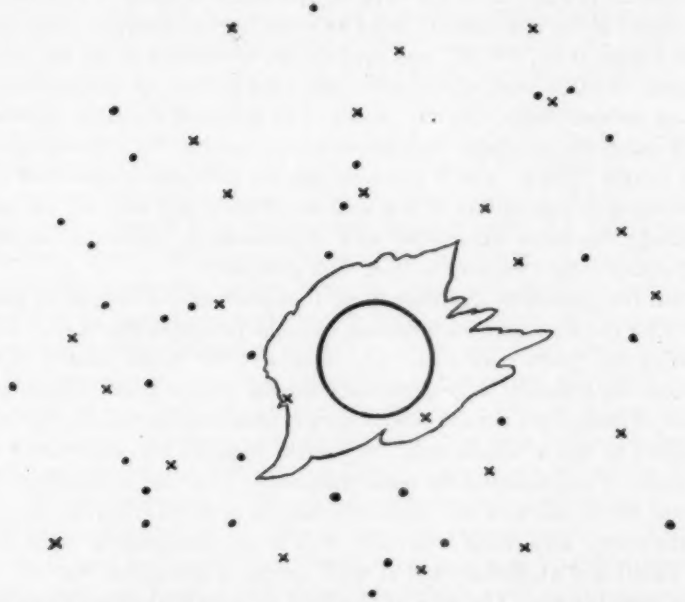

FIG. 3. *Eclipse and Check fields. The star images of the eclipse and of the check field are intermingled all over the plate. In the imaginary diagram, the round dots represent the stars of the eclipse field: the x's the stars of the check field. On the plates the images of the two groups are indistinguishable except through their measured positions.*

Now, these checks fields could also be used to determine the plate constants; and plate constants, so found, would be entirely independent of the eclipse stars and their unknown displacements. Both Campbell and Trumpler found such plate constants: but they made only partial use of them in reducing the eclipse stars. These eclipse stars, whose deflections were sought, were reduced by a combination of constants. Some of these plate constants were adopted directly from the check

fields others were found from a combination of stars in both fields, and the "scale value" was found directly from the eclipse stars, from the very stars whose positions Campbell and Trumpler were attempting to find.

In the 1923 paper for the 15-foot cameras few details are given. It is stated, however, that the "second order terms determined by the solution of the check field" were adopted for the eclipse region. The remaining constants were apparently determined from some of the eclipse stars. These so-called "reference stars" were selected from those "more than 2° distant from the sun's center"; and the remaining constants and the "scale value" were determined from the assumption that these reference stars were free from all displacements of every kind and character. The resulting displacements of the other stars, Campbell calls "relative displacements." This is a perfectly correct procedure, provided it is always remembered that these relative displacements are purely fictitious, and may bear no resemblance to the actual displacements. If all the stars, including the reference stars, have, in fact, an outward displacement (similar to that indicated by the yearly or Courvoisier refraction), then the scale, determined by Campbell's method, will be too small, and a fictitious outward displacement of all the inner stars will result: a linear fictitious displacement resembling the Einstein radial deflection. If on the other hand, these reference stars of Campbell have, in reality, an "inward" displacement, then the resulting scale will be too large and all the inner stars will appear to be radially displaced towards the sun. By taking other groups for "reference stars" and by determining the scale upon the assumption that the average displacement of such stars is zero, entirely different fictitious effects can be found.

Trumpler followed the same general method for finding the "scale" to reduce the plates of the 5-foot cameras, but he adopted a somewhat different method for finding the other plate constants. The constants determined from the check field for the second and third order terms were used: but the "final values of zero corrections" (position of the center of the plate) were determined from all the stars of the eclipse field, except those within 1° of the sun. The remaining constants and the scale value were determined, by least square solutions, from 55 selected "reference stars" of the eclipse field 5° and more from the sun's center. The basic assumption involved in this method is that the average displacement of these 55 reference stars is zero. The displacements of all other stars, as tabulated, are thus purely relative. Now while

Campbell, in the paper of 1923, correctly labelled these displacements as "relative"; Trumpler, in the paper of 1928, calls them "Observed Displacements."

The relative displacements of the different stars, so determined may differ greatly from the real, or absolute displacements; and a line, or curve drawn through such relative displacements may bear no resemblance to the line, or curve, which best represents the actual displacements. These real, or absolute displacements can, however, be found from the relative displacements by applying proper corrections. But such corrections can only be determined when the absolute displacement of the reference stars is known. As soon as such absolute displacement is known, then the corrections can either be calculated by a linear scale correction, or they can be shown diagrammatically by drawing a new base line. Such new base line runs from the origin to and through a point representing the negative displacement of the reference stars.

All of this may be illustrated by the data and diagrams of the Lick Observatory Expedition. The annexed diagram, from a tracing of Fig. 2 of Trumpler's paper,[18] shows the relative radial displacements of the stars of the eclipse field as found from the plates of the 5-foot cameras. Each dot represents a weighted group mean, derived from five or six stars for the inner groups, and from twenty, or twenty-two stars for the outer groups. These relative displacements appear to show some general conformity to the corresponding Einstein curve of relative displacements, as shown by the dotted curve. But such apparent conformity may be entirely fictitious, and this curve of relative displacements may bear no resemblance to the curve, which represents the absolute, or true displacements. To turn these observed relative displacements into the absolute displacements requires a knowledge of the absolute displacement of the reference stars. Different values for this displacement will give radically different results, as indicated by drawing different base lines for the absolute deflections. The first, A, represents the corrected base line, when the absolute deflection of the reference stars is exactly equal to the predicted Einstein amount, namely $0''.06$ of arc: the second, B, when this unknown displacement is found by independent observations to be $0''.50$ of arc.

In order to exhibit clearly the differing characteristics of these various curves of possible absolute displacements, the necessary corrections may be applied to Trumpler's observed relative values. This is done

[18] Trumpler: Lick Observatory Bulletin, No. 397; pages 150, 151.

in the following figure, in which the displacements, due to the Einstein assumption, are shown as full dots connected by a light full line; the displacements, due to the assumption that the reference stars are displaced by 0″.50, are denoted by x's connected by a line of dots and dashes; and the absolute displacements, calculated from the assumption of a displacement of 1″.00 for the reference stars, are given by asterisks connected by a line of dashes with double dots. The predicted Einstein deflections are shown by the dotted curve. A mere glance at this figure shows from what radically different sets of absolute displace-

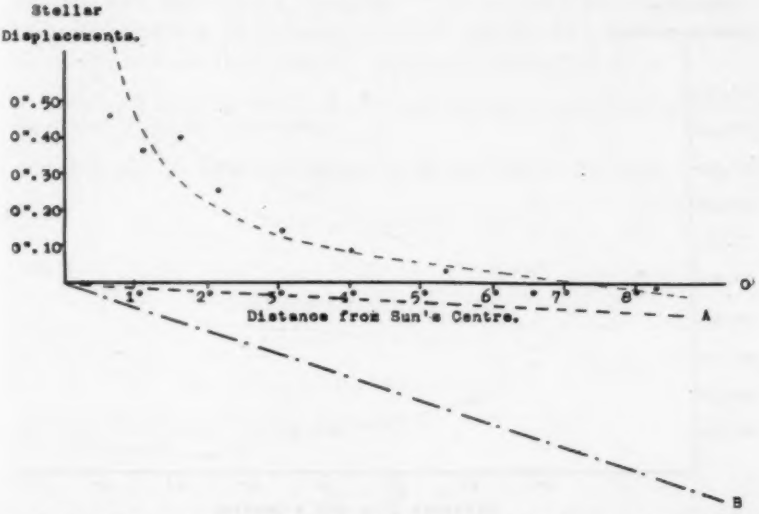

FIG. 4. *Relative and absolute displacements. The group means of the eclipse stars are plotted as functions of the star's angular distance from the Sun's center. These are relative to the reference stars if measured from the base line O: they are corrected for different assummed displacements of the reference stars if measured from the base lines, A or B.*

ments may be obtained the same set of "relative displacements." The absolute displacements may resemble the Einstein deflections, decreasing in size with distance from the sun; they may be nearly constant; or they may, in fact, increase with the distance as indicated by the upper curve. And these new curves may even give far more consistent results than those obtained by Trumpler on the Einstein assumption.

Now it should be perfectly clear, from all this, that the "relative displacements," in and by themselves, do not give any information

as to what the absolute displacements of the stars really were. Such absolute displacements depend entirely upon the true deflection of the reference stars, and such deflection must be obtained by some independent means. It cannot be found from the relative displacements, or from any measures of the eclipse stars themselves. Yet this fact appears to have been lost sight of in the methods adopted for the reduction of the eclipse plates. As we have seen, the absolute radial displacement of any star can be found from the corresponding relative displacement through a scale correction, derived from and depending

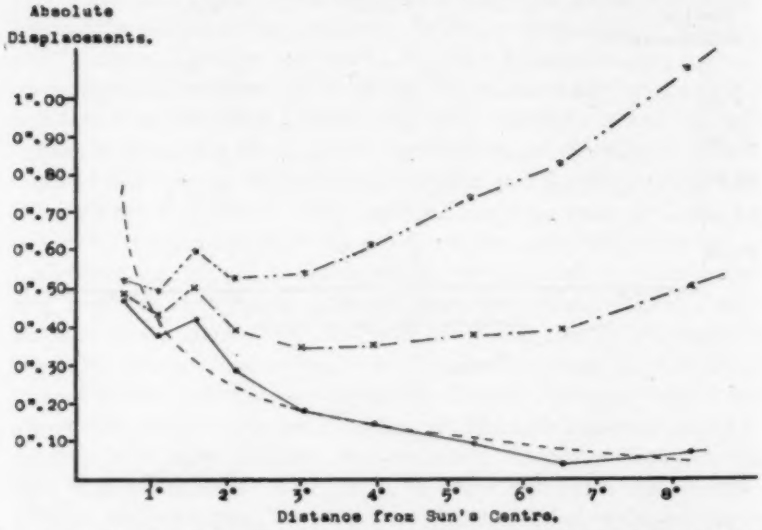

FIG. 5. *Absolute displacements corresponding to different assumptions. Full dots joined by full lines: the Einstein assumption. Crosses joined by line of dashes and dots: assumption of 0".50 Asterisks joined by line of dashes with double dots: assumption of 1".00.*

upon the true displacement of the reference stars. This may be put in mathematical form as:

$$\text{Absolute displacement} = \Delta r + \alpha d$$

where,

d = distance of the star from the sun.

α = scale correction, proportional to the unknown displacement of the reference stars.

In this equation, however, both α and the absolute displacement are unknown; and their values cannot be found from such equation. The addition of similar equations, for other stars, does not help the problem,

for each such equation introduces an additional unknown (the absolute deflection of such star); and in any such set of equations there will always be one more "unknown" than there are equations. But, if we assume that the unknown absolute displacements are connected by some definite law, so that, as soon as one is known, all the rest can be calculated, then the left hand side of our various equations can all be expressed in terms of a single unknown quantity. The many equations, derived from the various stars, will then involve only two unknown quantities; and such equations can be solved. This is the method adopted by Campbell and Trumpler. They assumed that these various unknown absolute displacements follow the Einstein law; and they wrote this law into their formula. Campbell[16] states this, as:—

"Adopting Einstein's prediction that the displacements are inversely proportional to the distance d from the Sun's centre,"

and then put the unknown deflection of each star in the form:

$$\text{Absolute displacement} = \beta \frac{R}{d}$$

where

β = light deflection of an hypothetical star at the Sun's limb.

R = radius of the Sun's disc.

And wrote his equations in the form:—

$$\beta \frac{R}{d} = \Delta r + \alpha d$$

From a set of such equations for each plate, involving the measured relative displacements of all the eclipse stars, Campbell determined the values of α and β by a least square solution.

Campbell and Trumpler apparently assume that this method gives, for each plate, the correct value of the scale correction, α, and the absolute, or observed value of the deflection, β, of an hypothetical star at the sun's limb. They show that the various plates taken with the 15-foot cameras give values for β varying between $1''.48$ and $1''.85$; and from the plates of the 5-foot cameras Trumpler finds values running all the way from $1''.01$ to $2''.54$. And by combining all these results with "adopted" weights, Trumper finally concludes that the mean β of

[16] Campbell and Trumpler: Lick Observatory Bulletin, No. 346, page 51.

all the plates "coincides exactly with the prediction of Einstein's Generalized Theory of Relativity." But all these results are of no value whatsoever. The stellar displacements obtained by this method are not the "absolute," or "observed" displacements of the stars: they are merely hypothetical displacements depending upon and derived from the two basic assumptions written into the formulas of reduction. These assumptions are:—

1. The light deflections follow Einstein's law and decrease in size with increasing distance from the sun's centre.

2. The unknown and unmeasured displacement of the reference stars shall be such as to make the "corrected displacements" of all stars agree most nearly with the Einstein prediction.

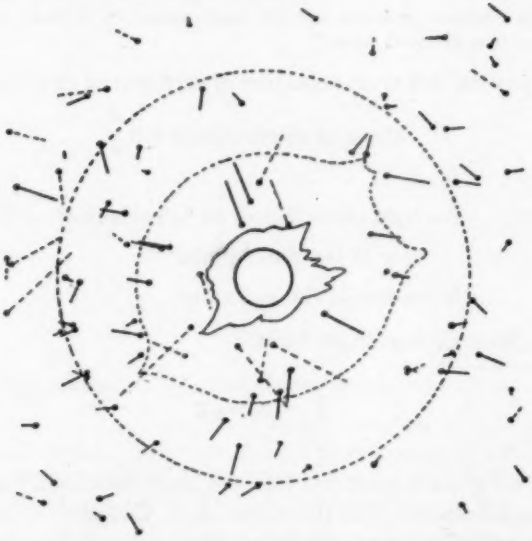

FIG. 6. *The eclipse observations. This figure shows the relative displacements of 92 stars as observed by Campbell and Trumpler at the total solar eclipse of September 21, 1922, and is from a direct tracing of the Star Chart in Lick Observatory Bulletin. No. 346. In the chart, however, the displacements of 23 stars were omitted: these have been added to made the above diagram complete and are shown by dotted lines.*

Neither Campbell nor Trumpler, so far as these papers show, made the slightest attempt to test, or verify these basic assumptions. Yet if these basic assumptions are invalid, their entire conclusions are invalid. Campbell's paper (Bulletin No. 346) does not contain a trace of evidence to support the assumption that the deflections decrease

with distance, does not contain a single fact to show what the radial deflections of the 38 reference stars really were. The solution of the equations based upon the assumptions, gives a deflection for these stars of some 0″.22 of arc, agreeing almost exactly with the Einstein prediction. But such value was written into the equations. For all the elaborate tables and calculation contained in this paper, no one has the vaguest idea as to what this deflection really was. Such stars might have been displaced by half (0″.5) a second, a second (1″.0), or even more outward: they may have all been displaced inward.

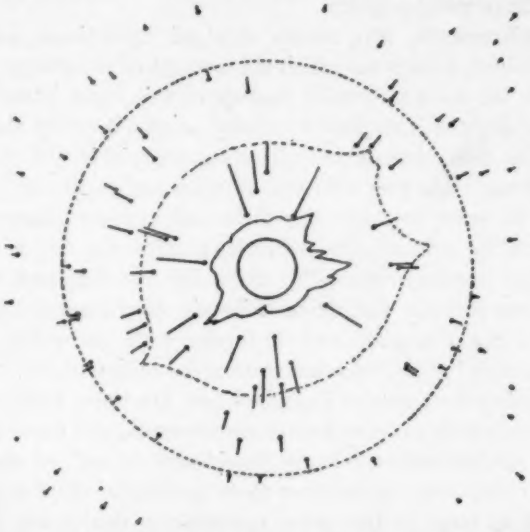

FIG. 7. *The Einstein prediction. This figure shows the displacements, according to the prediction of Einstein, of the 92 stars observed by Campbell and Trumpler. These theoretical displacements are not the full predicted star displacements, but are the relative displacements derived from them, by Campbell and Trumpler, by applying the scale correction used in their Star Chart.*

The same criticism applies to Trumpler's elaborate paper (Bulletin No. 397). His conclusions and results are invalid, unless and until independent evidence can be produced to support his similar basic assumptions. He wrote the Einstein law and the Einstein deflection into his formulas; and he naturally got the Einstein law and the Einstein deflection out of his reductions.

By such methods and by scales so determined, Campbell and Trumpler reduced their observations and found their apparent confirmation of Einstein's prediction. Their papers contain long tabulations, dia-

grams, and charts showing the results of the intricate calculations and least square solutions. The most complete chart of results is that given in the first paper, and which is here reproduced. The original chart, however, was not complete. For while 92 stars were observed and measured by Campbell and Trumpler, the chart shows the relative displacements of only 69 stars. The displacements of the missing 23 stars were taken from the tables in Campbell's paper, and have been added to the original chart. These added displacements are shown by dotted lines, so as to clearly distinguish the added material from that shown on the original diagram.

The displacements, here shown enlarged 2250 times, are relative to the 38 reference stars for which the average displacement was made zero. Now the most noticeable feature of this chart is the fact that hardly a single star is displaced radially, as called for by the Einstein theory. The deflections of only 14 stars are within 10° of radiality; those of 12 stars are over 45°; and 18 stars are displaced inward, not outward. In order to make this difference between observation and theory perfectly evident, the deflections called for by Einstein are shown in the annexed figure. To make the two diagrams fully comparable, however, the theoretical Einstein displacements have been reduced, by the Campbell and Trumpler scale correction, so as to become "relative" to the reference stars of the eclipse plates.

In discussing their results Campbell and Trumpler followed the example of Eddington and the British astronomers, and treated all these transverse displacements as mere "accidental errors" of observation. A glance at the chart shows that these tangential displacements are very nearly as large as the radial components, which are treated as real. In fact, a tabulation of the radial and tangential displacements of 61 stars, the images of which were designated as "good" by Campbell, shows the following:—

Total Radial Displacements	17″.35
Total Outward Displacements	14″.60
Total Tangential Displacements	10″.81

It seems rather arbitrary to dismiss the 10″.81 tangential displacements as accidental errors, and to accept the 14″.60 outward radial displacements as representing actual observed stellar deflections. And further it must be remembered, that these tangential components are practically independent of the scale used in reducing the plates; while the radial components depend absolutely upon the adopted scale. A slight change in the scale value will completely eliminate all outward

displacements and cause the deflections of all stars to be inward towards the sun; but such change will have no appreciable effect upon the tangential displacements. It would seem, therefore, that, in disregarding these tangential components, Campbell and Trumpler threw away the only, real, actual results of their entire labor.

Neither Campbell nor Trumpler give any reason for discarding these tangential displacements. They merely state that they "are entirely accounted for by the accidental errors of observation." A possible explanation of such action may lie in the fact that considering the

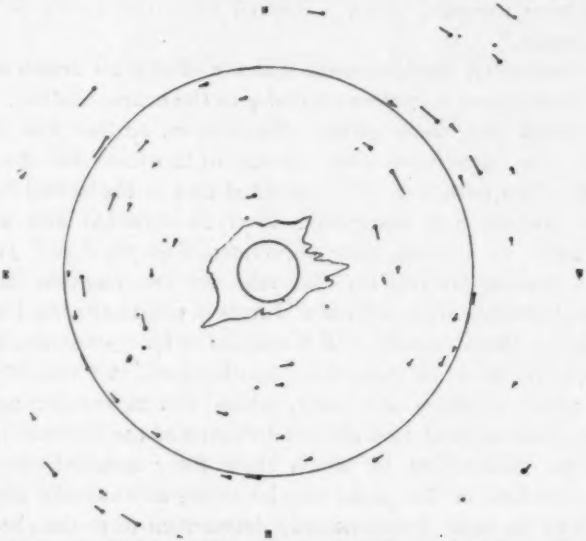

FIG. 8. *Tangential displacements. These tangential displacements are from a direct tracing of the Star Chart in Lick Observatory Bulletin, No. 346. They are independent of small changes in scale value. They show a decided symmetry, and for 61 stars they amount to 10".81. Campbell and Trumpler, however, consider them as accidental errors of observation and disregard them.*

algebraic sign of these tangential displacements, they divide as $+5''.01$ and $-5''.80$. These practically cancel, and make the algebraic sum of all tangential displacements only $-0''.79$ of arc. But this is perfectly normal for a displacement of any type, which varies in size with position angle. If these displacements vary in accord with either a sine or a cosine curve, the algebraic sum of them all would necessarily be zero. That these tangential displacements do thus vary with position angle is clearly apparent from even a casual inspection of the following figure, which is from a direct tracing of Campbell's star chart. In the

north-east quadrant practically every deflection is counter clock-wise; in the south-east quadrant, this is directly reversed; with but one or two exceptions, the deflections are all clock-wise. In the westerly quadrants, this symmetry is not so marked, and can only be brought out by an analysis of the actual displacements. The line of symmetry is not exactly east and west, but runs from a point somewhat north of west to a point some 10° or 20° south of east; a line roughly agreeing in direction with the passage of the shadow over the eclipse station. And, further, it is noticeable that these displacements increase with distance from the sun. Such a marked effect can hardly be deemed as "accidental."

These tangential displacements indicate clearly an action of some kind; probably some refractive effect due to the eclipse shadow.

The claims that these eclipse observations sustain the Einstein prediction rest, thus, upon only one-half of the measured data. Not only that. The reduction of the selected half of the actual measures depends, literally and absolutely, upon unsupported and untested assumptions. In making their reductions, Campbell and Trumpler wrote the Einstein law into their formulas and assumed that the actual radial displacement of the reference stars is in practical accord with the requirements of the theory. It would seem that some independent test should be made of these basic assumptions. Unfortunately the original paper (Bulletin No. 346), which was announced as giving results "in exact accord with the requirements of the Einstein theory," contains no information by which these basic assumptions can be tested. Nowhere in the paper can be found any definite statement in regard to the scale, independently determined from the check field stars. Such scale was undoubtedly found, and it would seem that such scale might be used to test the basic assumption in regard to the reference stars. In the 1928 paper, however, Trumpler gives certain data from which this scale can be found. In his elaborate attempts to explain away discrepancies between his measures and the requirements of the theory, he gives the values of the scales used for the check field stars; or rather he gives the differences between these scales and those determined from the eclipse stars themselves. For the 15-foot cameras this difference amounted to 0″.415 for each degree (1°) of distance from the sun's centre: for the 5-foot cameras to 0″.133 per degree. When these differences are corrected for the temperature effects, due to uniform expansion of the cameras caused by the higher temperature of the telescope houses at the time of the eclipse, then Trumpler

showed that these differences would be reduced, respectively, to 0″.375 for the 15-foot cameras, and to 0″.113 for the 5-foot cameras.

When these independently determined scales are used, it would appear that the average displacement of the reference stars, used for the 15-foot cameras, should be approximately 0″.95 instead of the 0″.22 of Campbell's basic assumption; and that the average displacement of the 55 reference stars, used for the 5-foot cameras, should be some 0″.82 instead of the 0″.07 of Trumpler's basic assumption. All semblance of accord with the Einstein prediction vanishes. The apparent accord, announced by Campbell and Trumpler, was based upon assumptions of practical "accord" for the reference stars, which have no basis in fact.

Not only are the displacements of the reference stars changed by the use of these scientific scales, but so are the radial deflections of all the other eclipse stars. For the 15-foot cameras, the average radial deflection is now seen to be 0″.88; being slightly smaller for the inner stars and increasing slowly to some 1″.08 for stars at approximately 3° from the sun. For the 5-foot cameras, this average is 0″.61; the displacement of the inner stars being some 0″.50, while that of stars 7°.4 from the sun is considerably larger, or 0″.82: Both sets of plates agree in showing, first a slight decrease in the size of the deflection followed by a gradual increase of the deflection with increasing distance from the sun. This is an effect directly contrary to the Einstein law. And there is substantial agreement between the average size of the deflections as determined by the two sets of plates:—0″.88 and 0″.61.

All of this is shown on the following diagram, which represents the radial displacements of the 61 principal stars of Campbell's star chart, as calculated by the "check field" scale given by Trumpler. These radial displacements are free from all assumptions as to reference stars, as to scale, and as to any law of increase or decrease with distance from the sun. It is at once apparent that these radial displacements bear no resemblance to the "relative displacements" of Campbell's chart, or to the corresponding predicted displacements of Einstein. When combined with the tangential displacements, as shown in Figure 8, these independently determined radial displacements would seem best to represent the actual observations.

In his 1928 paper Trumpler states that:—

"It had never been the intention of the observers to use the check field photographs for determining the scale of the eclipse photographs."

and dismisses the possibility of using these straight-forward, independent, and scientific values of the scales with the single sentence:—

> For, if the scale value of the check field (corrected for temperature expansions with the air temperature) were used for the reduction of the eclipse field, the two instruments would furnish quite incompatible results for the light deflections of the stars between 2° and 3° from the Sun's center." (5-foot cameras: 0".48, 15-foot cameras: 0".94)

So far as mere figures go, this statement is correct: the check field scales do give the results as stated. But Trumpler accepts as perfectly satisfactory equally "incompatible" results obtained by his method of

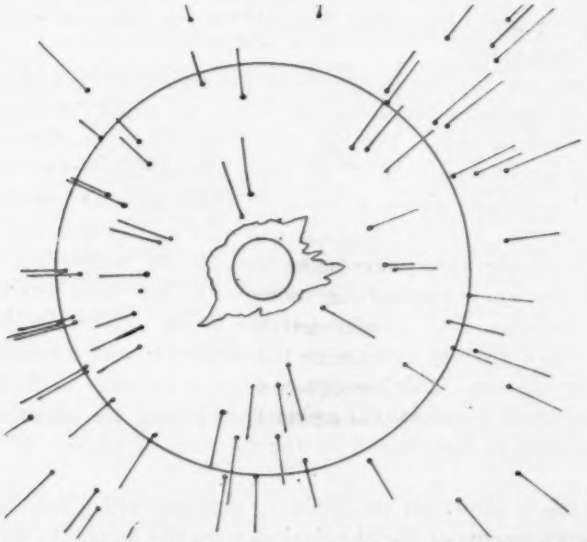

FIG. 9. *The radial displacements. These radial displacements were calculated by the scale found for the "Check Field" corrected for temperature expansions with the air temperatures, as given by Trumpler in Lick Observatory Bulletin, No. 397. These displacements are, therefore, free from all assumptions in regard to the eclipse stars, and would seem best to represent the actual observations. They do not show the slightest resemblance to the Einstein prediction, Figure 7.*

writing the Einstein prediction into his assumptions and into his formulas of reduction. For his tabulated results show that the 15-foot cameras give an Einstein deflection of 0".67 for stars at 0°.64 from the sun, while the 5-foot cameras give only 0".47 for similar stars. And for stars distant 1°.65 from the sun, the large cameras give 0".28, while the small cameras give the very much larger deflection of 0".41. As a matter of fact the "check field" scales give far more consistent results for

the two sets of plates, than do the scales based upon the Einstein assumption. The displacements calculated by the check scales for the 5-foot cameras are uniformly smaller than those for the 15-foot cameras: the percentages for different "Group Means" ranging between 48 and 77. But not so for the displacements as tabulated by Trumpler for the Einstein scales. Some of the "Group Means" for the 5-foot cameras are smaller and some are larger than those for the 15-foot cameras: the percentages vary from 70 to 146.

Regardless of their actual size, regardless of whether they be reduced by the Einstein assumption of Trumpler, or by the scientific check scale, these radial displacements display a most interesting characteristic. They vary in size as to the eclipse progressed, and this variation is shown by both sets of photographs. It is shown by the mean results derived from all the stars, and it is shown by the displacements of the individual stars. Taking the 15 best stars, photographed with the 5-foot cameras, one finds that the plates taken at the beginning of the eclipse give decidedly larger deflections than do the two plates taken near the end of totality. Nine out of the fifteen stars show this, and the average deflection of the whole fifteen, as shown by Trumpler's tables, was 0″.43 for the earlier plates, and only 0″.18 for the last two plates. Trumpler, himself, noted this fact, for he states:

"the two first plates of the 5-foot cameras give a large, the two last ones a small value for the light deflection at the Sun's lims."

From his tabulation of final results one finds for this progressive change in value:—

Plates	M15, N18	2″.19
	M16, N19	2″.10
	M17, N20	1″.18

This change in the size of the deflection, with the passing of the shadow, is similar to that found in the results of the British astronomers; and would seem to indicate the result of some atmospheric disturbance. But Trumpler dismisses these apparently clear-cut results with the mere statement that they must be "accidental."

But, neglecting all such evidence of change in the actual deflections, and discarding all tangential displacements, Trumpler combines the average radial displacements of the two instruments. And in doing this, he again writes the Einstein law into his formulas. The two sets of measures cannot be directly combined, for they are "relative" to two entirely different sets of reference stars. "Assuming with Einstein,"

however, the law of decreasing size of the absolute displacements, Trumpler found, from the 5-foot cameras, that some 26 stars at 2°.52 from the sun were displaced 0″.15 of arc in relation to his reference stars at 7°.4. He assumed this 0″.15 to be the absolute displacement of the reference stars of the 15-foot cameras; and, from this assumption, found a scale correction to reduce "the results of the 15-foot cameras to the system of the 5-foot cameras." With this new scale Trumpler found new values for the radial displacements of the stars of the 15-foot cameras, and then, through a system of adopted weights, combined these new results with those of the 5-foot cameras. The results of this process of combination he tabulates and diagrams as "Group Means of Observed Radial Star Displacements (Mean of the two instruments)." Such final results are in practical accord with the Einstein prediction.

Thus, even in this final combination of results, Trumpler, as in all his other calculations and reductions, assumes the truth of the very law the eclipse expedition was organized to test. And he naturally finds that his results are best represented by the law from which they were obtained.

CONCLUSIONS

1. Eclipse observations cannot furnish a valid test of the relativity theory, for such observations do not involve a single one of the theory's basic postulates. The sole concept involved in the predicted star displacements is that of a possible "gravitational retardation of light."

2. The eclipse observations, as heretofore carried out by the various expeditions, are inadequate to test even this one question. The gravitational effect, if present, is of the second order as compared to many necessary corrections and disturbing factors.

3. Not a single expedition, so far reporting, has made use of effective checks or controls to eliminate the effects of temperature changes upon its instruments, or to determine the possible effects of the abnormal conditions of the atmosphere during the eclipse.

4. Not a single expedition, so far reporting, has made a systematic study of all the data obtained. In the South American eclipse of 1919 less than 15% of the actual measured data was used in obtaining the announced result: in the Australian eclipse of 1922 only about 50% of the data was used. All non-radial components of the actual measures were discarded as "accidental errors."

5. The data, thus selected, was then reduced by methods and formulas, which "assume with Einstein" the existence of the predicted

radial displacements and of the Einstein law of decrease with distance from the sun. No attempt has yet been made to test the validity of these basic assumptions.

6. There is not the slightest observational evidence to support the Einstein assumptions thus written into the formulas and methods of reduction. On the contrary, the only observational evidence available—the evidence furnished by the independent check fields photographed on the eclipse plates—would seem to show that these assumptions are invalid.

7. The actual stellar displacements, when freed from all assumptions, do not show the slighest resemblance to the predicted Einstein deflections: they do not agree in direction, in size, or in the rate of decrease with distance from the sun.

8. The actual measured displacements, if real, can best be explained by some refractive effect of the earth's atmosphere: by a combination of the Courvoisier effect, of day-light refraction, and of temperature effects caused by the passing of the eclipse shadow.

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New York, N. Y.

A COMPARISON OF LABORATORY AND SOLAR WAVE LENGTHS*

By Keivin Burns

All are familiar with the fact that a red shift of the solar spectrum lines has been predicted as a result of Einstein's theory of relativity. At the solar surface, this predicted shift follows the equation $\nu = 2.13 \times 10^{-6}$ and is ordinarily given in A.U. But expressed in these units, the shift varies from end to end of the spectrum, so in this paper it seems preferable to express all such values in parts per ten million, which gives convenient quantities, and makes all parts of the spectrum directly comparable. Expressed so, the predicted Einstein shift to the red will be twenty-one parts per ten million.

This paper was presented at the Fourteenth Annual Meeting of the Optical Society of America at the invitation of the Program Committee which asked the writer to discuss the question: Does the evidence now available support the contention that the solar spectrum lines are shifted to the red by this amount? A general survey of the conditions made to decide whether the problem is soluble by present methods and instruments, shows that in this respect the spectroscopic field is much more fortunately situated than is the case with the other two astronomical proofs, so called, of the relativity theory, viz. the motion of Mercury's perihelion and the deflection of light rays by the sun's gravitational field as shown on eclipse photographs. In both these other methods, many authorities feel that the quantities sought are of an order too small to be certainly defined by present methods and apparatus.

The spectroscopic method suffers from no such limitations. In brief, we may trust the mean values of a number of good spectrum lines, determined with the best modern precision, to about three parts in ten million, or to about one seventh of the shift to be sought. Remembering that St. John[1] and other observers[2] have found that all solar lines are

* Text of a paper presented by invitation at the annual meeting of the Optical Society, Ithaca, N. Y., October 25, 1929, as part of a Symposium on "Experimental Data Bearing on the Theory of Relativity." In publishing this paper, the Optical Society wishes to take no part in the arguments pro and con relative to Einstein's Theory.—Ed.

[1] St. John, Aph. J., *67*, pp. 117–161; 1928.

[2] Burns, Meggers, A. O. Publ., *6*, pp. 105–124; 1925. Burns, Kiess, A. O. Publ., *6*, pp. 125–139; 1927. Burns, A. O. Publ., *6*, pp. 141–150; 1927. Warga, A. O. Publ., *6*, pp. 151–157; 1928.

shifted to the red by varying amounts, it might seem to a worker in another field that there was nothing more to be said. If our spectroscopic observations were about five times less accurate than they really are, the problem would long since have been regarded as closed, and a triumph for Einstein's theory. But the accuracy of these modern spectroscopic methods is such that we have, so to speak, found out too much. It would be quite possible, by choosing only certain stronger lines of solar intensity 6 to 8, to average up a large number of shifts from such lines and find almost precisely the Einstein shift as predicted.

But the matter is far from being so simple as that, and so satisfactory to the supporters of the Einstein theory. Our spectroscopic observations are in a sense too good, and we find that this red shift varies in a striking manner with the intensity of the solar lines. Very strong lines are all shifted more, the weaker lines less, some of them much less, than predicted. It seems at present quite probable that if it were possible to determine their positions with the greatest modern accuracy, the sharpest solar lines, they are all very weak, would show no shift at all. This difference in behavior between weak and strong lines will be referred to as the intensity equation. There can be no satisfactory theory of this red shift until we can arrive at some certainty as to the normal positions of the solar lines if the effects of this intensity equation were removed. At present this is only partially possible.

In my own part of this symposium,* it will be necessary to discuss three aspects of the problem. First, and more briefly than the subject demands; the attainable precision of modern spectroscopic methods, and their adequacy for the purpose. Secondly; evidence showing the very striking and unmistakable intensity equation, in that weak solar lines are shifted to the red by an amount decidedly smaller that that required by the Einstein theory, while the strong lines are shifted more than this amount. In the third section, we shall discuss the question; are there any possible theories that can adequately explain away the difference in shift shown by this intensity equation, and still leave the Einstein prediction as a reasonably valid explanation. Some of us do not believe that this can be done, but one's opinion on this point will depend on his interpretation of highly technical and involved considerations of solar levels and spectrum multiplets.

The discussion throughout will be based largely on new data secured in recent years by cooperation of the Bureau of Standards and the Alle-

* The experimental data bearing on the theory of relativity.

[3] Kiess, Bur. Stds. Jour. Res., *1*, pp. 75–90; 1928.

gheny Observatory in a program of precise wave-length determinations of lines in the spectra of the sun and of various vacuum arcs. Whenever the sun and laboratory are compared, the laboratory source is the vacuum arc, regarded by us as of primary importance because of the very low pressure existing at the solar surface.

It is by no means an easy matter to obtain wave lengths which are accurate to .001A, and we can not here enter into all the precautions which must be taken. For a full discussion of instruments and methods, the reader is referred to A. O. Publ., *6*, pp. 105-124; 1925.

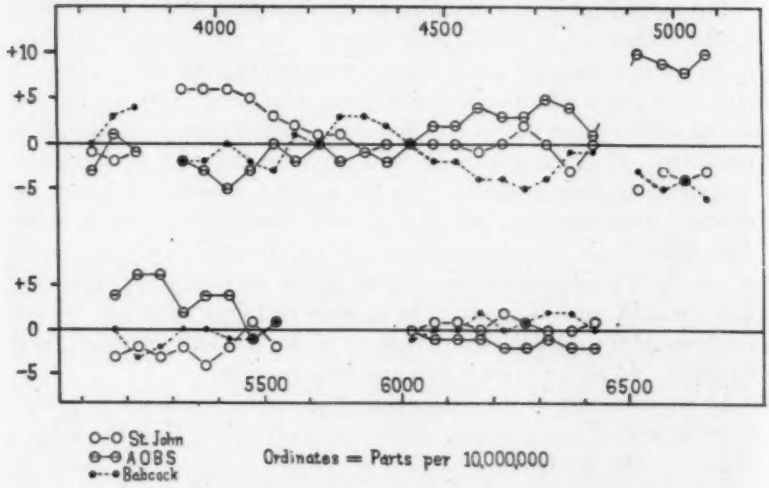

FIG. 1. *Comparison of three series of solar wave lengths.*

The general accuracy of modern work in the solar field is perhaps best summed up in Fig. 1, which compares the work of St. John,[5] of Babcock,[5] and that of the A. O. B. S. When it is recalled that the ordinates of this diagram are parts per ten million, the agreement is remarkably good excepting for the short region 49-5300A. In the violet, the extreme range is a part in a million at worst, half of that at best, and the probable error is about three in ten million. In the red the probable error is only one in ten million. It seems, then, entirely safe to answer in the affirmative the question as to the adequacy of present methods in evaluating a shift of the predicted value, twenty-one in ten million which is represented by the distance between the zero lines of ordinates.

[4] Brown, Aph. J., *56*, pp. 53–70; 1922.

[5] International Committee, Trans. Int. Astr. Union, *3*; 1928.

In fact, a shift of a half or a third this amount would be indicated with great certainty if existing and if unaffected by other causes.

The A. O. B. S. wave lengths refer to integrated solar radiation, and were all compared directly with neon standards, by interference methods. St. John used a plane grating, and iron arc in air standards, observing the center of a large solar disk. Babcock used the interferometer, and iron standards, observing sometimes the center of the solar disk, sometimes using integrated light.

TABLE 1. *Sun minus vacuum* Ti *arc, parts per 10,000,000.*

λ/Int	0	1	2	3	4	5	6	6	10	
36-4300A	4	3	10	10	17	10	—	9	—	TiI
	18				10				31	TiII
	14				18				25	FeI
43-4900A	14	14	18	19	13					TiI
				22	16	20	24			TiII
				18	24	24	26			FeI

In Table 1 are shown mean values of differences sun minus vacuum titanium arc. The laboratory wave lengths are those of Kiess,[3] supplemented by those of Brown[4] and of Burns (unpublished). Some of the solar wave lengths have been reobserved, the others are taken from Rowland and corrected by means of the International Committee Report for 1928.[5] The few lines of TiII which are available show about the same amount of red shift as do the lines of FeI of the same solar intensity in the same region of spectrum. This is in disagreement with the findings of St. John,[1] who found the TiII lines to be shifted farther to the red than iron lines of like solar intensity.

In Table 2 are collected the means of all the solar iron lines which have been well observed by modern methods, and for which precise laboratory wave lengths are available. The list is confined to those regions of the spectrum in which the solar lines have been observed in fair or good agreement by three observers, excepting that Babcock's observations of the infrared have been included. Remeasured lines of intensity 1 are not numerous, and lines of intensity 0 are lacking, in the region shorter than 5800A. For wave lengths shorter than 4600A the strong lines, intensity 7 and greater, have red shifts greater than the predicted twenty-one parts in ten million, the weak lines are shifted less than this amount.

Since they are not so very numerous, and since no change in the in-

tensity equation with wave length is apparent in this region, the lines of the red and infrared are all grouped together. For these regions, computed laboratory wave lengths are used for intensity 0; for lines of intensity 1, 2, and 3 the laboratory wave lengths are part computed and part observed. The laboratory wave lengths of lines of intensity 4 and greater are all observed; some of them have been measured in four series of observations on twenty or more plates. The infrared lines of iron and of calcium were observed in only two series, with a limiting order of 45000. The long wave solar iron lines show only half the predicted shift of twenty-one parts per ten million and we must give great weight to this region because of the proximity of the standards and the perfect agreement of three series of solar observations.

TABLE 2. *Reobserved solar wave lengths, 1928 I. A. Scale. Sun minus vacuum iron arc, parts per 10,000,000.*

λ/Int	0	1	2	3	4	5	6	7	8	10	15	20
36–4000A	—	18	16	16	17	16	19	24	24	23	22	23
40–4300	—	—.	12	15	18	16	19	—	25	25	27	28
43–4600	—	14	15	17	16	18	27	—	28	32	37	
46–4900	—	16	12	14	16	20	17	—	16			
59–8800	8	4	8	9	9	10	12	14	12	19		
	Sun minus calcium vacuum arc											
60–8600				13	9	2	—	—	16	14	15	48

The few lines of calcium are appended mainly to show that a large shift in the long wave region would not be overlooked if present. The means for CaI agree with iron as well as can be expected when the small number of lines is taken into account. CaII shows a shift of forty-eight parts per ten million for lines of intensity estimated at about 15. This compares with the shift of forty-four parts per ten million found by St. John for the CaII lines H and K in the violet, the agreement being of course accidentally good.

At this point the question naturally arises whether the intensity equation may be due to instrumental causes, and the data in Table 3 indicates that this may be in part the case. We have here the differences St. John minus Rowland and A. O. B. S. minus Rowland, all three having been reduced to the same scale as the 1928. I. A. While the A. O. B. S. wave lengths are small, both in comparison with St. John and with Rowland for intensities 1 and 2, the A. O. B. S. values are larger than Rowland's for lines stronger than intensity 4. St. John's wave lengths of the strong lines are notably smaller than Rowland or A. O. B. S. in the

region 43-4500A. This purely instrumental discrepancy is of the sort that may be expected when lines are somewhat unsymmetrical, and may perhaps be explained by the fact that St. John used greater effective resolution, and so measured closer to the maxima of absorption of broad lines which are slightly unsymmetrical.

We may further illustrate the possible importance of purely instrumental causes as operative in causing the intensity equation by Fig. 2. It should be emphasized that the possibility of securing wave lengths free from intensity equation are greater for emission lines than for absorption lines. Let us assume for instance, a multiplet, and that all the lines of whatever intensity have the same type of dissymmetry. In

TABLE 3. *Reobserved solar wave lengths minus Rowland. Parts per 10,000,000.*

λ/Int	1	2	3	4	5	6	8	Observer
42-4300A	—	+7	+2	+2	+5	0	+5	St. John
	—	−2	0	—	0	−5	+2	A.O.B.S.
43-4500	−5	−2	−2	−2	−5	−9	−8	St. John
	−5	−7	0	+5	+5	+2	+5	A.O.B.S.

an emission spectrum we should make exposures of different length and measure each line at what we supposed to be the optimum density, and no doubt the wave lengths would closely represent the maxima. If, on the other hand, the lines are observed in absorption, it is quite possible that a 4-4 line may show the same width as a 1-0 line with an effective path 100 times as long; that is, a path containing 100 times as many atoms in condition to absorb this line of such a multiplet.

This is shown schematically in Fig. 2, where we assume two lines of the same shape both when symmetrical and unsymmetrical, but which differ by a factor of ten in the amount of absorption experienced. It is evident that when the lines are unsymmetrical, the stronger line will be observed with greater relative wave length than would be the case were it symmetrical. Such conditions may explain some of the differences between different series of solar wave lengths, since the values would depend to some extent on the resolving power and exposure, yet no choice of density or instrument will permit two such lines to be measured with the same relative wave length as their maxima would show in emission.

Further light on the red shift of the weaker lines may be obtained by a consideration of the wave lengths derived from computations based on the atomic level differences of other measured lines of high accuracy. For example, some five hundred lines in the spectrum of the vacuum

iron arc in the region 28-8800A have been measured at the Allegheny Observatory,[6] and the differences derived from these atomic levels permit some additional twelve hundred wave lengths to be computed. A few selections are shown in Table 4 which compares such computed with the observed wave numbers, and enables us to study the relative wave lengths of weak and strong lines. Such lines as 4291.4 are ideal for testing whether the weak and strong lines have been measured on the same scale, since they arise from weak combinations of levels which are themselves derived from strong lines. This particular wave length

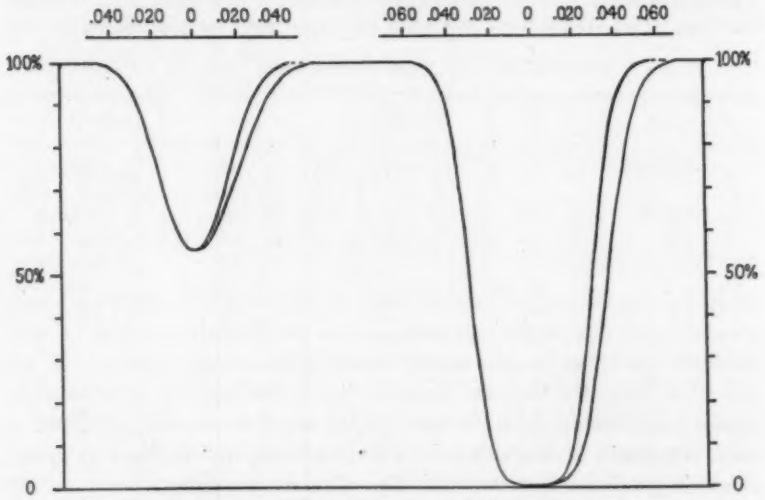

FIG. 2. *The displacement which accompanies increased absorption in the case of an unsymmetrical line.*
Wave lengths in sun and arc.

is slightly too short, and a study of all such lines available indicates that such weak lines average two parts in ten million too long, which is quite satisfactory as a confirmation of the small discordance between the scales of weak and strong laboratory lines.

We may then summarize the portion of the discussion thus far as follows;—Modern spectroscopic methods, with due precautions, are of ample power to detect a red shift of the predicted amount, or even one-third as large. There is also no doubt that the stronger lines of the solar spectrum are shifted to the red, but this shift is found to be inextricably

[6] Burns and Walters, A. O. Publ., *6*, pp. 159–211; 1929.

bound up with an intensity equation. There is good reason to believe that some of the weakest and sharpest lines of the solar spectrum have little if any red shift, certainly their shift is far less than the predicted Einstein shift. There is also some reason for believing that this varying red shift is partly due to instrumental causes; if this proves to be true in any considerable measure, we shall be forced to give a definite negative answer to the question set at the beginning of the paper and say that

TABLE 4. *Observed and computed wave numbers.*

Wave length	Wave number		Int. Sun	Terms °=odd
	Observed	Comp.		
4260.474	23464.986	991 D	10	$a^7D_5^\circ - a^7D_5$
64.206	444.450	462 W	3	$a^5F_4^\circ - 31W_3$
66.965	429.292	289 G°	3	$a^3G_4 - b^3G_4^\circ$
71.153	406.320	323 D	6	$a^7D^{\circ\circ} - a^7D_4$
71.760	402.993	995	15	$a^5F_4 - a^3G^\circ$
82.403	344.831	829 S°	5	$a^5P_3 - a^5S_2^\circ$
88.146	313.563	565 G°	1	$a^3G_3 - b^3G_3^\circ$
91.464	295.543	541	2	$a^5D_3 - a^7P_4^\circ$
94.125	281.105	100	5	$a^5F_4 - a^3G_4^\circ$
98.034	259.928	932 M	2	$2M_4 - 16R_4^\circ$
4299.234	253.436	433 D	4	$a^7D_4^\circ - a^7D_5$
4299.234	253.436	433 D	4	$a^7D_4^\circ - a^7D_5$
4302.185	237.489	485 M	2	$2M_4 - 24R_4^\circ$
05.447	219.885	873 M	3	$3M_2 - a_5S_1^\circ$
07.902	206.652	655	6	$a^5F_3 - a^3G_4^\circ$
4315.085	23168.022	025 S°	4	$a^5P_3 - a^5S_2^\circ$

there is no adequate observational evidence for a red shift in the solar spectrum of the amount and character predicted by the Einstein theory.

There would be but one escape from this conclusion;—the formulation of a theory of solar conditions rather than line dissymetry which would give an adequate and satisfactory theory sufficient to explain the small shift of weak lines and the excess of red shift of strong lines, while at the same time retaining an average red shift of the right size to fit Einstein's theory.

There is only one such theory which at present deserves serious notice, that propounded by Dr. St. John,[1] and the concluding portion of this paper will be devoted to a consideration of this theory, and the main lines of evidence which seem to render this hypothesis untenable. The

basis of St. John's theory may well be quoted in his own words; "The concept that the Fraunhofer lines in the spectra of the sun and stars refer to definite levels is steadily gaining acceptance and application. The observational evidence for this concept rests upon the concordant results from solar rotation, flow near spots, flash spectra, difference between the spectra of limb and center, progression in excitation potential, and the observed decrease in the strength of the general magnetic field with heights above the photosphere at which the lines used have their origin."

St. John's solution of the difficulties raised by the existence of the intensity equation are then about as follows; the solar lines of great intensity are assumed to have their source at great heights in the solar atmosphere with this source falling toward the center of the sun at rates of one or two kilometers per second in the case of Ca+, giving such lines a larger shift to the red through this added velocity of recession. The weak lines, on the other hand, are supposed to be emitted by low lying vapor which is on the average rising toward the exterior of the solar atmosphere; this small velocity of effective approach toward the observer diminishes the wave length, and accounts for the fact that weak lines show less than the predicted shift.

It is evident that such supposed velocity effects should be a maximum at the center of the solar disk, while near the limb such motions of the source would be nearly at right angles to the line of sight, and hence of very much smaller apparent magnitude. At the Allegheny Observatory with the cooperation of Meggers,[7] we have measured the relative displacements of weak and strong lines, including Ca+, at such a position on the solar disk that the radial velocities of such ascending and descending currents should be only forty percent of that at the center of the disk. At this point on the disk the so-called limb effect is negligible, and the spectrum is practically indistinguishable from that at the center of the disk. According to St. John's theory, the intensity equation here should have been only forty percent as great as at the center of the disk, yet we found it to be the same in both places. There are some reasons for believing, however, that the discrepancy may be more apparent than real;—the question can perhaps not be settled in the present state of our knowledge of solar absorption.

A far more serious and to me unanswerable objection to St. John's theory rests in a word, upon the fact that the modern multiplet theory

[7] Meggers, A. O. Publ., *6*, pp. 13–44; 1919. Burns, unpublished

of the spectrum gives us both weak and strong lines from the same atom, statistically speaking. Of one thousand atoms in the state a^5D_4, a definite percent will change to $a^5F_5°$, a lesser definite percent to $a^5F_4°$, etc. It seems paradoxical to assume as a possibility that a very strong line of a multiplet can originate at a high solar level, where St. John postulates the descending movement, while a weak line of the same multiplet originates only at the lower solar level in ascending vapor.

TABLE 5. *Composite $^5F^5D$ multiplet, violet*

Term	D_4	D_3	D_2	D_1	D_1
	25	Solar Intensity			
F_5	1500	Solar Level (km)			
	0	Evershed Effect*			
	25	Red Shift			
	7	20			
F_4	700	1200			
	+9	+2			
	23	24			
	5	8	10		
F_3	600	750	1000		
	+13	+6	+3		
	16	24	24		
		5	8	8	
F_2		600	750	750	
		+13	+6	+6	
		16	24	24	
			3	5	6
F_1			low	600	700
			+18	+13	+12
			16	16	19

* Flow near sun spots.

Table 5 shows some of the phenomena which St. John uses to fix a definite solar level to a line of given intensity. In the case of any real multiplet, some of the lines are affected by solar companions, and some of the data are lacking. So after examining all the available observations, a composite multiplet was set up, and this will illustrate the phenomena better than fragments of several multiplets. This composite multiplet is not intended to be representative of the mean of all F-D multiplets only, but of all multiplets in the spectrum of FeI, S-P to G-H, including

[4] St. John, Aph. J., *37*, pp. 322–353; 1913.

triplets, quintets and septets, Data on solar rotation and limb effect are lacking. The magnetic field was found to vary with line intensity, and the solar level was assigned from the intensity, so this adds nothing new to the discussion. In step with the excitation potential, the strong diagonal progresses in line intensity, and therefore with supposed difference in solar level. But note that in each column of the diagram the excitation potential is the same, while the line intensity has its maximum variation, and the trend of other data parallels the intensity. The level from flash spectra as shown in line 2 of each bracket is high for 5-4, lower for 4-4 and lower yet for 3-4, in step with the solar intensity. The lines 3-2, 2-2, 1-2, show the effect better, the 1-2 being too "low" to be observed, As some would have it, the transition 5-4 takes place at all altitudes up to 1500 km above the photosphere, while the transition 4-4 does not take place above 700 km. It is far more probable that the ratio of 4-4 to 5-4 transitions at 1500 km is nearly the same as in the vacuum arc, but the exposure of the flash is too weak to show the 4-4 at 1500 km elevation, as would be the case if a short exposure were made in the laboratory. The intensity of the high level line is always greater at the low level than is that of the supposedly low level lines. The flash observations are therefore not competent to show that lines of the same multiplet are not emitted at the same solar level. Comparing different multiplets of the same element, or better, of different elements the case is quite otherwise. The intensities of the magnesium triplets fall less rapidly with increasing solar level than those of the iron quintets of the same region of spectrum, and a comparison of iron and helium shows a still greater difference.

For flow near spots the appearance of the diagram is even more weird. The 5-4 jump is made at zero velocity with respect to the umbra (or the observer); the 4-4 transition is made in gas that is approaching or receding from the umbra (or observer) at moderate speed; the 1-2 jump is made at maximum speed. It is difficult to see how any condition connected with the rather small difference in the velocity of the gas can interfere with the ratio of the number of 5-4 and 1-2 transitions in such a degree that one is made as often as normal and the other not at all. Unsöld[9] explains this "curious" phenomenon by supposing that a dissymmetry of the absorption lines is introduced by the conditions surrounding a spot, and that the observer measures some part of the line other than the maximum of absorption, the distance from the maximum depending on the line intensity. This seems to be entirely possible,

[9] Unsöld, Aph. J., *69*, pp. 275–286; 1929.

and in normal regions of the sun's surface there is no doubt sufficient activity to make the lines somewhat unsymmetrical. As in the case of solar level, the lines of Ca+ undoubtedly have a different behavior from that of FeI. There is no objection to the theory that calcium is drawn into a spot while lanthanum is flowing out. But the writer is insisting that lines of the same element and of the same multiplet of this element cannot be supposed to have their source in gas that has a different velocity for each line.

The strong lines are accompanied by large shifts, the weak diagonal shows smaller displacements. In the velocity explanation of the intensity equation, the effect of the disturbance is placed at zero for lines of intensity 6 so as to arrive at the desired red shift of twenty-one parts per ten million. This procedure appears to the writer to be artificial and unconvincing. It would seem to be just as logical to place the effect of any of these other phenomena at zero at the same line intensity, but that is certainly impossible. Instead of postulating different velocities for the sources of the different lines in the same multiplet, the writer prefers to consider these lines to be all alike excepting for intensity, with the difference in red shift arising from a slight lack of symmetry.

To recapitulate; there seem to be the following objections to accepting as proven the predicted red shift of twenty-one parts per ten million for all the solar lines.

First; the sharpest lines show very small shifts, and the shifts of lines of all intensities are small in the region of the spectrum where on the basis of probable error the observations are the best.

Secondly; the striking intensity equation appears to be due in part to instrumental error and to be tied up with this error in such a way that the elimination of the error would leave the shifts smaller than at present supposed.

Third; St. John's velocity explanation of the intensity equation appears to be untenable in the light of atomic theory. An alternative theory, assigning the intensity equation to line dissymmetry, supposes the normal position of the solar lines to be even nearer to the laboratory values than the weak lines are found to be. And it seems to the writer that any explanation of the cause of the phenomenon should place the normal position of the lines, not in the middle of the shift, but at one end of the intensity equation, probably on the end of least shift.

While these are serious objections to accepting the Einstein prediction as verified, it is well to defer judgment until certain gaps in our

knowledge of solar and laboratory wave lengths have been filled. If interest in the red shift will induce spectroscopists to study solar lines with the highest possible resolving power, to measure the weak lines shorter than 5800A, and to observe the spectra of many more vacuum sources; then the results of these investigations will undoubtedly justify the large amount of attention that has been directed toward the red shift. These are matters for the future: For the present, although several text books are teaching our beginners that the proof of the red shift rests on at least as firm a foundation as that of the law of gravity, the writer thinks that the scientific mind does not usually accept as clearly proven a thesis whose proof requires the sort of treatment of the observational data that is necessary to bring the red shift of all solar lines to exactly twenty-one parts per ten million.

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THE OBSERVED MOTION OF THE PERIHELION OF MERCURY*

By Herbert R. Morgan

According to the law of gravitation the planets move in ellipses having the sun at one focus. The longitude of the planet, its mean motion, the dimensions of the ellipse and its position in space are called the elements of the orbit. These elements are all slowly changing due to the attractions or perturbations of the other bodies in the system. The perturbations are functions of the masses and elements of the planets, some of them being periodic and some of them secular.

From a series of observations of a planet extending over a considerable length of time there may be deduced the elements of the planet for some given epoch, and the variations of these elements with the time from that epoch. The latter are known as observed variations. Also knowing the masses and elements of the planets, the variations of the elements of any one planet due to the perturbations of the other planets may be computed from theory. These are known as the theoretical variations. Now quite generally such observed and computed variations agree, but there are at present a few small disagreements in the motions of the inner planets.

The largest discordance, that between the observed and computed rate of revolution of the major axis of Mercury's orbit, was pointed out in the middle of last century by the great mathematical astronomer Leverrier. Leverrier made an exhaustive study of the observations available at his time and derived a comprehensive system of tables of motions of the bodies in the solar system. He found a motion of 565″ a century in the longitude of perihelion of Mercury from the observations, while the motion due to the attractions of the other planets as computed from theory was only 527″, or a difference of 38″.

The work of Leverrier was superseded by the still more exhaustive work of Newcomb, who spent his life in the stupendous task of a general solution of the motions of the solar system, using observations of the Sun, Moon and planets extending over hundreds of years. And this sys-

* Text of a paper presented by invitation at the annual meeting of the Optical Society, Ithaca, N. Y., October 25, 1929, as part of a Symposium on "Experimental Data Bearing on the Theory of Relativity." In publishing this paper, the Optical Society wishes to take no part in the arguments pro and con relative to Einstein's Theory.—Ed.

tem is so coordinated, the masses and elements of the planets, the motions of the ecliptic and equator with reference to standard motions of the brighter stars, the numerous astronomical constants involved, that a change in any one element means a disturbance in many others. Newcomb pointed out that a change of the mass of Venus by one seventh of its amount would satisfy the motion of Mercury, but that such a change was impossible on account of its inadmissable effects on other motions.

In 1912 the secular variations of the four inner planets were recomputed by Doolittle using revised elements and masses and an entirely different method of computation. The perturbations of Mercury were also computed by Hill. The four computations by Leverrier, Newcomb, Hill and Doolittle, using two independent methods, agree very closely, upholding Newcomb's conclusion that there is no error in the theory of computation of the perturbations. The theoretical variations will be changed as the masses, elements, and reduction constants are changed, or by the added terms for masses not now allowed for but known to exist.

From Newcomb's reductions the difference between observation and theory in the motion of the perihelion of Mercury is 42.6″. Accepting the more accurate computation of Doolittle, and including Newcomb's later correction to his precession, this residual must be changed to 45.4″. An empirical term was added in the construction of the tables of Mercury such that they represent a correction of 45.9″ to Doolittle's computed value.

A mass of modern observations has accumulated since Newcomb's work, and while no definitive corrections to his system are available, numerous provisional comparisons have been made.

For the purposes of this paper provisional corrections to the tables have been worked out using three series of observations taken at the U. S. Naval Observatory from 1900 to 1925, including about 1000 observations, and the results of two series of observations from the Cape Observatory from 1907 to 1916, including about 500 observations, together with certain data from recent reductions of Sun observations at Cape, Greenwich and Washington. The mean correction for 1911 from these five sets of elements, to the longitude of perihelion in the tables, is found to be 2.3″ with a probably error of 0.3″. For a more definitive solution the results from these 1500 observations would be combined with the 5000 older observations in a general solution for a longitude and its variation, but for the present we may accept the longitude as determined by Newcomb for the mean epoch of his observations, 1865, as correct, and the one just determined as correct for 1911, the difference,

2.3″, giving the correction to the motion in the tables in the elapsed 46 years. This corresponds to a correction of 5.0″ a century, giving the difference between observation and theory in the secular variation of the longitude of the perihelion of Mercury the value 50.9″. From Newcomb's reductions of the transits of Mercury a longitude of the perihelion has been deduced for the mean epoch of the transits, 1820, which compared with the longitude at 1911 gives a residual 49.8″, agreeing with the 50.9″ found as above.

A few figures may be given as to the uncertainty of the results under consideration.

Of the 530″ secular change in Mercury's perihelion, Venus causes 276″ or more than half. The masses of Venus used by Leverrier and by Newcomb differ by 1-50 of its amount, corresponding to a difference of 5″ in Mercury's motion. One of the most accurate means of determining the mass of Venus is from its action on Mars. Recently two accurate solutions for the orbit of Mars have been made; the one using all meridian observations up to 1912; the other using heliometer observations up to 1924, and yet these two series give masses of Venus that differ up 1-50 of its amount, or 5″ in the motion of Mercury. This may be partly due to systematic errors in the two kinds of observations, and partly solutional.

The different determinations of the solar parallax give differences in the Earth's mass corresponding to 2″ in the motion of Mercury.

Newcomb found a difference of 8.5″ in the motion of the perihelion of Mercury from two solutions, the one using 5000 meridian observations extending over a hundred years, the other using these observations combined with two hundred years of transits over the Sun's disc. Here is a difference of 8.5″ due to systematic errors of different kinds of observations and solutional errors in handling the results.

Newcomb found a change in this motion of 7.3″ according as he used Leverrier's elements of the Sun or used his own elements.

Evidence is accumulating that the system of stars to which the precessional motions of the equator and ecliptic are referred has a rotation of its own. The motions in the solar system depend upon the gravitational precession of the system only.

Mechanically the rotating Sun should be oblate by 0.05″. So far observations have not shown this, but it is within the errors of observation. This would correspond to 4″ in the motion of Mercury's perihelion.

There is a common fluctuation in the longitudes of the planets of

several seconds a century the effect of which upon the motions under consideration is not clear. An element longitude is affected by a fluctuation in the equinox.

The residual next in size of discordance to that for the perihelion of Mercury is that for the node of Venus. For the purposes of this paper provisional corrections to the elements of Venus have been worked up using about a thousand observations taken at the Naval Observatory from 1900 to 1911, and the results from some 1200 observations taken at the Cape and Greenwich Observatories from 1907 to 1923. The mean correction at 1911 to the longitude of node is 3.5″. Considering the tables correct at 1863 this corresponds to a correction of 7.6″ a century to the motion in the tables. The masses of Venus and the Earth used in the tables need correction such that the above quantity must be increased to 13″ for the residual between observation and theory in the secular variation of the node of Venus.

Somewhat more definitive elements of the Sun and Mars have been published and an examination of them has been made. While the residuals for the four planets are in some cases quite different from those given by Newcomb, and amount in some cases to three or four times their probable errors, the two outstanding residuals are still those in the perihelion of Mercury and node of Venus both of which are larger than found before.

By considering the attractions of masses of matter known to exist around the Sun explanations of the residual motions of the inner planets have been given by a number of authorities. The conclusion has been that masses sufficient to explain all the residuals would be brighter than the observed phenomena. It may be significant that Dr. Jeffreys found that the best representation of the other residuals required a residual for Mercury one and a half times the 43″. This agrees with the present results which indicate a much larger value.

The Relativists are definite in stating that the Einstein theory calls for certain motion in the perihelia only without explanation of residuals in the eccentricities or orbit planes. Abstracting the Einstein motion of 42.9″ from the above residual for Mercury still leaves a residual four times its probable error.

A new general solution of the solar system will doubtless give quite different residuals. The suggestion of Professor Poor seems pertinent, namely, that the various causes suggested will each contribute a share in explanation of these motions.

It has been indicated very briefly that there are uncertainties of several

seconds in the present residuals between observation and theory in the secular variations of the inner planets. Newcomb continually pointed out the uncertainties in his results, and it is rather pathetic that at the close of his last work he states "It is therefore necessary . . . . that the secular motions of the elements be completely rediscussed."

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WASHINGTON, D. C.

Journal of the Society of Motion Picture Engineers. Published monthly by the Society of Motion Picture Engineers at Easton, Pa. $12.00 annually.

The first issue of the new Journal of the Society of Motion Picture Engineers made its appearance early in January. Hereafter it will be published monthly, replacing the semi-annual Transactions, formerly published by the Society. Recommendations for the new Journal were passed upon at the Society's last convention in Toronto.

The new journal is the same size as the old Transactions, and will contain from 120 to 150 pages each issue, according to present plans. The Journal will be strictly scientific or technical in nature, carrying only articles of the highest scientific, technical and engineering quality, and shall be kept free from semi-popular, commercial, trade, and advertising types of article.

The contents will consist of articles of the following classes: papers read at the Society's semi-annual meetings and those read at meetings of local sections of the Society; other papers authorized by the publication committee; reprints of papers from both foreign and domestic sources, book reviews; patent notes and news of the Society's activities, etc.

For the present the Journal will be published by the Journal committee, of which L. A. Jones is chairman, acting as temporary editor. However, it is planned to appoint a permanent editor who will be assisted by a board of associate editors.

Papers of importance in the first issue include the following titles: The Sound Film Situation in Europe; Early History of Motion Picture Cameras for Film Wider than 35 mm; Rectangle Proportions in Pictorial Composition; The Optical Problems of Wide Film Motion Pictures; Some Practical Aspects of and Recommendations on Wide Film Standards; Acoustic Control of Recording for Talking Motion Pictures; Some New Aspects of Reverberation; Camera and Projector Apertures in Relation to Sound-on-Film Pictures; Report of Standards and Nomenclature Committee; Abstracts; Reviews; Editorials and Miscellaneous Notices.